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EDITED BY

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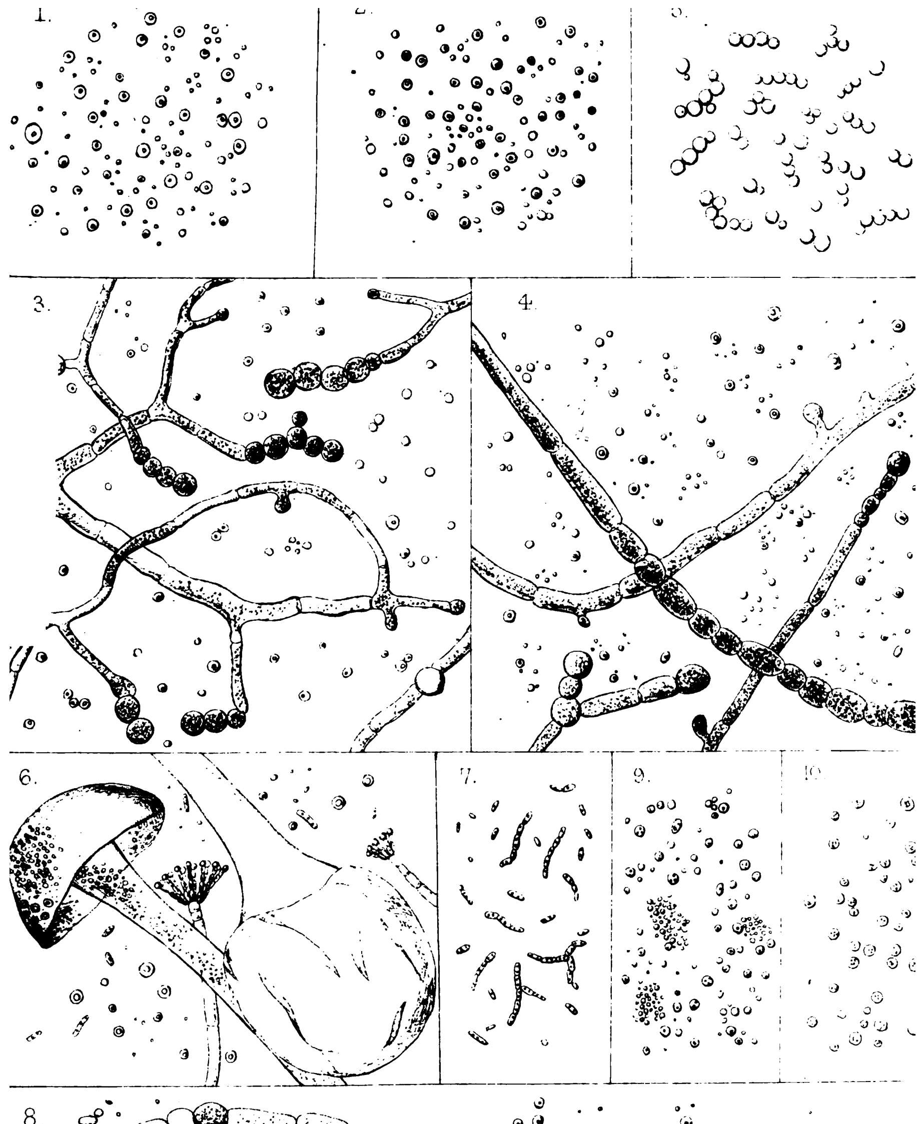
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THE

MONTHLY MICROSCOPICAL JOURNAL.

JULY 1, 1870.

I.—*On Fungi and Fermentation.* By JAMES BELL, F.C.S., &c.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *June* 8, 1870.)

PLATE LV.

THE first series of experiments to which I propose to refer is of a somewhat fundamental character, and relates to the behaviour of nitrogenized bodies in sugar solutions.*

Albumen and Gluten.—It being generally understood that albumen and other nitrogenized substances are subject to spontaneous or natural fermentation, a series of experiments was instituted to determine the character or nature of the fermentation which nitrogenized bodies derived from different sources would give rise to.

The albumen of egg was first selected for experiment. To a solution of cane sugar a small quantity of egg albumen was added, and the sample was placed in a chamber which was kept at a temperature of 75° Fahr.

In a few days numerous minute transparent cells or spores were found to be diffused throughout the liquid; these soon increased in size, but the largest were relatively much smaller than the cells of wort or wine ferment. The sample was examined on several occasions for alcohol, but great difficulty was experienced in the distillation on account of the frothing caused by the presence of the albumen. The largest quantity of alcohol† found in the liquid at any one time amounted to only about two-tenths per cent.

The liquid gradually became acid, and in two or three weeks was found to be full of *vibrios*. At the end of forty-six and seventy-four days respectively the sample contained 1·02 and 2·16 per cent. of acid, and on the latter occasion it contained no alcohol whatever, the acidifying power having become greater than the alcoholic.

The cells contained a bright nucleus, sometimes situated in

* These experiments are still in progress, the albumen being subjected to various modes of treatment.

† The alcohol in each case is given as absolute, and the percentage is estimated by volume.

the centre and sometimes near the coating. These are shown on Plate LV., Fig. 1.

Gluten was next selected for experiment. A sample was carefully prepared from wheaten flour, and a portion was introduced into a cane sugar solution. The gluten at first floated on the surface of the liquid, but in a few days it became much disintegrated and subsided to the bottom of the flask, and afterwards became gradually diffused throughout the liquid.

On the second or third day an action was observed in the sample, and small transparent cells were seen to be tolerably abundant, but they did not appear to possess much action. The sample was frequently examined for alcohol, but very little was found to have been produced; and at the end of thirty-four days it only contained 1·65 per cent. of alcohol and ·24 per cent. of acid.

The spores or cells in this case had a bright nucleus similar to that which characterized the egg albumen cells. The gluten cells are represented on Plate LV., Fig. 2.

Wheaten flour albumen was next tried, and in the first instance a cold-water flour extract was prepared, the starch and other insoluble constituents being separated by filtration. The clear filtrate containing the albumen in solution was divided into two portions; to one 21·80 per cent. of glucose was added, and to the other about 15 per cent. of cane sugar.

In a day or two after the samples had been prepared they were both found to be full of microscopic animals, of which some were ovoid in form, and others consisted apparently of several minute joints. The portion to which the cane sugar had been added soon became thick and syrupy, and it was apparent that the sugar was being converted into mucilage.

The transformation of the cane sugar in a cold-water flour extract was a result which was entirely unexpected, and it is probable that the change in the sugar was induced through the operations of the little creatures present in the solution, for they all appeared to be extremely active, and apparently devouring some substance in solution.

At the expiration of twenty-four days the sample was examined for alcohol and acid, and it was found to contain six-tenths per cent. of acid,* but no alcohol.

The portion of the extract to which glucose had been added became turbid and slightly mucilaginous; and at the expiration of twenty-four days the sample was examined for alcohol and acid, and it was found to contain ·72 per cent. of acid but only 0·17 per cent. of alcohol. There were present in the sample a few cells resembling those of yeast, but they appeared unhealthy and inactive.

* The acid is estimated as acetic.

From these results it was obvious that in order to study the ferment which wheaten flour albumen would give rise to, the albumen would have to be separated and introduced directly into the sugar solution.

A small quantity was therefore prepared by heating a cold-water flour extract, and separating the coagulated albumen by filtration. The albumen after having been well washed was introduced into a solution containing 15·62 per cent. of glucose. The sample was examined daily and was found to be free from microscopic animals, and remained so for about a week although the air had access to the liquor through a glass tube.

At the end of fourteen days the sample was examined for alcohol and acid, and was found to contain neither. In the meantime mycelium was observed developing throughout the albumen, and giving rise to transparent spores or cells with a bright nucleus similar to those that occurred in the other albumen sugar solutions.

The sample was again examined for alcohol at the end of a month and was then found to contain one-tenth per cent.

Another small quantity of albumen from flour was prepared by precipitating the albumen from the cold-water extract by alcohol. The precipitated albumen was then diffused through water which was afterwards heated for some time to drive off the alcohol, and about 15 per cent. of glucose was added to the albuminous liquid. On the following day, however, the sample was found to contain a considerable number of the microscopic animals which it was intended to get rid of. In fact a large proportion of these creatures present in the flour extract was enclosed in and carried down with the precipitate.

At the end of fourteen days the sample was examined for alcohol and acid, and was found to contain ·12 per cent. of acid, but no alcohol. Now it will be observed that the result in this case is different from that obtained in the experiment with the albumen which had been prepared by coagulation, in the one case there were microscopic animals present, while in the other they were absent.

This sample was again examined for alcohol at the end of twenty-eight days, when it was found to contain ·19 per cent.

In this experiment, the mycelium developed in the albumen was similar to that formed in the albumen prepared by coagulation.

Albumen was prepared from ground malt in exactly the same manner as in the case of the flour, and the like experiments were performed with it in sugar solutions with almost identical results. The albumen prepared from the malt extract by heating was also found to be entirely free from microscopic animals, while that obtained by precipitation abounded with them. The mycelium and spores developed in the flour and malt albumen are represented on

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Plate LV., Figs. 3 and 4. Some yeast cells, however, as might be expected, were found in the sample to which the malt albumen that had been precipitated by alcohol was added, and it was found to contain a larger percentage of alcohol than any other of the albuminous samples.

The experiments in the case of the malt were carried a little farther. Cane sugar was added to the malt extract from which the albumen had been separated by heating, and although the liquid in a short time afterwards was alive with minute creatures, the sugar was not converted into mucilage, and in the course of a few days natural fermentation set in.

The extract of malt from which the albumen had been precipitated by alcohol was also operated upon. When the precipitated albumen had been separated by filtration, the alcohol contained in the filtrate was removed by distillation, and to the residue about 15 per cent. of cane sugar was added. On the fourth day fermentation set in, and the ferment produced was the purest and finest specimen that I have seen, and the sample was entirely free from microscopic animals, &c.

In fact the animals and germs that escaped being carried down by the alcoholic precipitate appeared to have been subsequently destroyed by long boiling in distilling off the alcohol. At the end of ten days the sample contained 10·63 per cent. of alcohol, and only ·18 per cent. of acid; so that, after all the manipulation through which the extract was put, the natural ferment produced possessed considerable powers, and which were still unexhausted.

The various germs and organisms infesting grain are the most formidable enemies with which we have to contend in the fermentation of grain extracts. If these once obtain possession of the field, and gain the ascendency, they materially interfere with the success of the fermentative process.

A temperature of about 175° Fahr., at which grain extracts are usually prepared, is not sufficient to destroy the animals and germs; their vitality may be suspended, but if the subsequent conditions are favourable to their growth, they become revivified.

To determine the effect which boiling has upon the fermentation of a malt extract, a comparative experiment was made. An infusion of malt was prepared and kept for two hours and a half at a temperature between 170° and 180° Fahr. The extract was then divided into two portions, one of which was boiled for some time, and afterwards about 15 per cent. of glucose was added to each portion. At the end of ten days the portion which had been boiled contained 10·38 per cent. of alcohol and 0·27 per cent. of acid, while the other contained 0·78 per cent. of acid and only 6·26 per cent. of alcohol.

In the one case the yeast cells had had possession of the field

first, whereas in the other it was simultaneously occupied by a variety of organisms.*

A cold-water extract of barley meal also possesses the property of converting cane sugar into mucilage, but the yeast cells soon develop, and resolve the greater portion of it into alcohol and carbonic acid gas. This is a most interesting phenomenon, and it serves to convey some idea of the wonderful power and singular properties of the yeast plant.

Under the microscope the yeast cells can be seen in the mucilaginous mass, sometimes in bundles resembling a bunch of grapes, and sometimes in a chain-like form, and also that the multiplication of the cells is carried on by a process of budding, Plate LV., Fig. 5.

This mucilaginous form of solution will afford an excellent opportunity of studying the development and growth of the yeast plant, and be the means of finally settling the question as to its true vegetable origin. Cane sugar is not converted into mucilage in all extracts prepared from barley meal. In a great many cases, especially at this season of the year, and under the influence of a high temperature, the yeast cells begin to act soon after the preparation of the extract and counteract the formation of mucilage.

The formation of mucilage is also prevented if, before the addition of the sugar, the extract is boiled for about half an hour in order to destroy the microscopic animals present.

In cold-water extracts of malt, to which cane sugar has been added, the cane sugar is not converted into mucilage, as in the case of barley meal and wheaten flour extracts. There is at first a slight tendency to thickening, but there appears to be an opposing action set up in the malt extract which prevents the formation of mucilage.

With malt extracts prepared in the cold very fair fermentations can be obtained; but as the samples are primarily occupied by a variety of organisms, a large percentage of acid is produced, and considerable loss of alcohol sustained.

The results hitherto obtained point to the microscopic animals as being the proximate cause of both the transformation of cane sugar into mucilage, and the production of acid; and the change effected in the sugar especially tends to the inference that the action is a catalytic one. It is hardly possible to believe that these creatures could have devoured so large a quantity of sugar in so short a time, or that having devoured it they would have reproduced it in a mucilaginous form.

Moulds and Pus.—A few experiments have been made with certain moulds, and one with pus, to determine their respective actions in sugar solutions.

The study of the development and properties of moulds, like

* There has been a considerable difference observed in the number of animals and germs present in different samples of malt.

that of most matters connected with fungi, is undoubtedly a critical one, as is fully evidenced by the absence of an agreement of opinion on the subject.

My inquiry has been directed to the action of moulds rather than to determine their origin and mode of development, but as it is alleged by some that the blue mould of malt is the ultimate development of the yeast plant, I cannot help stating that I have failed in my experiments to identify it as such; on the contrary, the moulds appear to preserve their distinctive character, and to be identified with a more advanced stage of decomposition than the true yeast plant; and, as a rule, wherever they occur they leave unpleasant evidences, which is certainly not the case with the yeast plant.

It is not improbable that there is some relation subsisting between the yeast plant and certain of the moulds, but whatever the connection is, it is obviously not yet understood, and remains to be determined.

A quantity of blue mould with the pasty mass attached was collected from the surface of moistened ground malt, and introduced into a sugar solution containing 15 per cent. of glucose, and the sample was placed in a chamber maintained at a temperature of 70° Fahr.

On the third day the liquid was alive with microscopic animals, and carbonic acid gas was being eliminated. On the fourth day it contained ·18 per cent., and on the sixth day ·28 per cent. of acid, and ·17 per cent. of alcohol. On the ninth day it contained 1·34 per cent. of alcohol and ·42 per cent. of acid; and at the end of twenty-eight days 7·48 per cent. of alcohol and ·60 per cent. of acid.

On the seventh day the cells or spores were observed to be mixed in character, some elongated cells similar to those found in malt fermentations having sprung into existence; and it will be observed that it was subsequent to this that the alcohol began to be produced in an appreciable quantity, so that the probability is that some yeast cells were produced, for the addition of the mould crust was equivalent to adding a portion of malt extract.

At the end of twenty-eight days the liquid was drawn off, and the residue at the bottom was introduced into a fresh solution of sugar; and at the expiration of eighteen days the sample was examined, and found to contain about 2 per cent. of alcohol.

The mould up to this time not only maintained its identity, but it will be seen from the representation on Plate LV., Fig. 6, that it was propagating in the liquid in the brush or plume-like form, after the fashion of its aërial fruiting, and that the spores are distinguished from yeast cells by a central ring, instead of containing granular matter.

It will be noticed from the same figure on Plate LV. that a new acquisition was obtained, in the form of a fresh fungoid growth; and this circumstance is important in showing the length of time that spores may lie dormant in a solution before they become developed. There can hardly be a doubt that the spores which gave rise to this new form of fungus were originally present in the malt extract from which the mould had been taken, and that they remained inactive until the process of decomposition in the organic matter had reached a point which perhaps was not only favourable to, but tended to stimulate their development.

This, I believe, may be taken as an illustration of the difficulty that is experienced in making experiments with grain extracts, which are infested with animals and germs of various kinds to an extent that is something marvellous.

A quantity of mould from lemon juice was next obtained, and added to a solution containing 14 per cent. of glucose, and placed in the same chamber in which the preceding experiment was conducted. On the sixth day the sample was found to contain 0·91 per cent. of alcohol, and on the twenty-third day 8·44 per cent. of alcohol and ·24 per cent. of acid.

This mould was extremely persistent, and maintained its position on the surface of the liquid throughout the fermentation; while the mould obtained from the malt became in a short time broken up and diffused throughout the sample.

It is worth noting in the case of the lemon-juice mould, and I have remarked the same thing in natural wine fermentations, that the quantity of alcohol produced fully represents the percentage of glucose; whereas in the case of the malt mould the quantity of alcohol produced is less than it ought to be according to the proportion of the glucose present.

In Fig. 7, Plate LV., is represented a specimen of the cells or spores taken from the residue at the bottom of the flask.

A sample of pus was next introduced into a solution containing 7 per cent. of glucose, and the sample was placed in a chamber kept at a temperature of upwards of 70° Fahr.

On the seventh day the sample was found to contain small transparent cells or spores, and flocculent masses of mycelium were observed floating in the liquid.

At the expiration of sixteen days the sample was found to contain 0·65 per cent. of alcohol, and ·06 per cent. of acid; and it is somewhat remarkable how free the sample was from microscopic animals.

The mycelium and spores produced by the pus are represented in Fig. 8, Plate LV., and it will be observed that they are almost identical in character with the mycelium and spores represented in Figs. 3 and 4, on Plate LV., and produced from the flour and malt albumen sugar solutions respectively.

The like description of fungoid mycelium development is occasionally met with in boiled malt extracts, but in albumen sugar solutions it usually occurs in an isolated form.

It is somewhat curious that this fungoid development in albumen sugar solutions should resemble so closely in character the development of the *micrococcus* in Hydrophobia in one of its stages of cultivation by Dr. Hallier.

From these and the other results which I have obtained it would appear that many fungoid developments in sugar solutions tend to produce alcohol and carbonic acid gas, but the proportion of alcohol produced within a given time by various fungi differs greatly.

In this lies the great distinction between the yeast plant and other fungoid developments, the yeast plant usually makes its appearance in three or four days, and it sets to work with an extraordinary degree of energy producing alcohol in great abundance.

There appears to be a kind of localized action with most fungoid growths in sugar solutions, whereas the action of the yeast plant is general, and it seems to be essential that the material upon which it depends for nourishment should be in solution.

Malt and Grape Yeast Plants.—Various comparative experiments have been made to determine the relative fermentative properties of the yeast plants of malt and the grape.

A cane sugar solution capable of yielding 17·5 per cent. of alcohol was prepared, and brewers' pressed yeast was then added in the proportion of 2000 grains to the half-gallon, and at different stages of the fermentation fresh quantities of yeast were added. On the ninth day the sample contained 12·32 per cent. of alcohol, on the twelfth 14·15, on the sixteenth 15·57, and on the nineteenth day 15·79, and at the end of thirty-six days 15·91.

It will be seen that during the first nine days alcohol was produced at a rate over 1 per cent. each day, but that its production from that time was extremely slow, and that practically on the sixteenth day, when the sample contained 15·57 per cent., the vitality of the yeast cells was suspended.

When the extreme point is nearly reached the cells become contracted, but when transferred to a fresh sugar solution and placed in favourable conditions they soon recover and begin to work afresh with their usual vigour. Every precaution was taken to guard against loss of alcohol by diffusion, as it was found that when about 12 per cent. of alcohol was reached the loss of alcohol in an open vessel was equal to the quantity generated.

Several malt extracts were prepared and the saccharine value thereof was raised to certain points by the addition of glucose, and allowed to ferment naturally.

The malt extracts were prepared by infusion at a temperature of about 180° Fahr., and from what has been already stated I need

hardly refer to the difficulty which was experienced in obtaining a pure healthy fermentation from a malt extract prepared at this temperature, although the infusion was maintained at that temperature for upwards of three hours, and the addition of 30 per cent. of glucose operated as an antiseptic.

The most successful malt wort fermentation was one in which the glucose, including that naturally present in the malt extract, amounted to over 36 per cent., and the fermentation was conducted at a temperature ranging from 63° to 70° Fahr. No fermentation was visible until the sixth day, when a number of hazy cells, which indicate the commencement of fermentation, made their appearance. Soon after this the fermentation began to be brisk, and by the ninth day 10·96 per cent. of alcohol had been generated, or within 1·35 per cent. of the quantity produced in the same time in the cane sugar solution by the addition of yeast. Now this fact, is very interesting, as it serves to show the great facility with which glucose, as compared with cane sugar, is resolved into alcohol and carbonic acid gas, and to indicate the large additional expenditure of force which must be required to effect the two transformations in cane sugar by the fermentative process.

From the ninth day the fermentation began to abate; in the three days following 2·83 per cent. of alcohol was produced, and in the next sixteen days 2·60, making altogether 16·35 per cent. of alcohol, which is the highest amount that has been reached in the fermentation of malt extracts prepared in the way described. It should be noted, however, that in this case the fermented liquid contained over 7 per cent. of glucose, and that it is probable if the proportion of glucose had been less a higher percentage of alcohol might have been produced.

It may be proper to mention that from a series of experiments it has been found that alcohol and sugar combine to act as an antiseptic, and that by increasing the percentage of sugar the fermentative power of the liquid is proportionately decreased. It is upon this property of their combined action that a less quantity of proof spirit is required to be present in liqueur than in ordinary wines, to prevent their deterioration. For example, a properly-made liqueur wine, containing 20 per cent. of glucose and about 14 per cent. of alcohol would be perfectly safe for keeping, or indeed for removing, to any part of the world; but if the wine contained only 14 per cent. of glucose, to render it proof against deterioration the proportion of alcohol would require to be increased to about 17 per cent.

A specimen of the exhausted yeast cells taken from the fermented wort in which the highest percentage of alcohol was produced, will be seen on Plate LV., Fig. 9; and it will be noticed that the cells are very granular and much aggregated, properties which are to some extent characteristic of the exhausted malt ferment.

The experiments with grape "must" had to be made chiefly with English hot-house grapes, and as these were gathered in the months of December and January, the percentage of glucose was, as might have been expected, low; but, as in the case of the malt extract, the percentage was increased to the required point by the addition of pure glucose.

In the fermentation of grape "must" the chief enemy to be encountered is the development of mould spores, which, when once established, affect the flavour of the wine, and in many instances render it unfit for use as a beverage.

As an illustration of this a sample of wine was made from English hot-house grapes, and it would have been a very fair sample considering the conditions under which it was made, were it not that it possessed a musty odour. On the second day after the juice was placed in the chamber for fermentation, a white mould, *Oidium Tuckeri*, formed on the surface, and attempts were made to remove it, but in vain, as it rapidly diffused itself throughout the juice. On the following day the yeast cells made their appearance in great numbers, and they soon took possession of the field and rapidly suppressed the further development of the mould on the surface. It is true that the ferment afterwards was never pure, but still the wine yeast cells continued to work until they had produced upwards of 17·68 per cent. of alcohol.

The grape ferment according to my observations is extremely pure and homogeneous in character, and if the conditions are favourable for its growth and development it does its work well. It works with great steadiness and possesses a degree of persistence which does not belong, at least so far as I have seen, to the beer ferment.

In most of the wine fermentation experiments the husks were nearly all fermented with the juice, and everything was adopted which was thought would add to the fermentative power of the "must." In one instance the percentage of glucose was made up to about 36 per cent., and the juice was placed for fermentation in a chamber at a temperature of 65° Fahr. On the fourth day fermentation was visible, and on the tenth day 10·20 per cent. of alcohol had been generated; on the twenty-third day 18·54 per cent.; and at the end of ten days more the percentage had increased to 18·65 per cent., which is the highest point that has been reached in natural wine fermentations. The cells, notwithstanding the amount of work which they had performed, were even at this point bright and globose, and maintained their entirety.

A sample of these exhausted cells was taken a few days ago from the residue of a sample of the wine, and their present appearance after the lapse of five months will be seen on Plate LV., Fig. 10.

Another sample of juice was similarly treated to the foregoing, but fermented at a temperature ten degrees higher. The fermentation in this case was visible on the third day, and at the end of fourteen days 17·40 per cent. of alcohol had been generated in the "must;" and at the expiration of a month, the temperature being continuously maintained at 75° Fahr., the proportion of alcohol had increased to 18·26 per cent. The yeast cells were much shrivelled, and some appeared to have burst; in fact, altogether the higher temperature was clearly less favourable for good, steady, healthy fermentation.

In another wine experiment the percentage of glucose in the juice was made up to about 40 per cent., and the fermentation was conducted at a temperature of about 65° Fahr.

The juice was prepared from the same sample of grapes as that employed in the first wine experiment above described, and the two samples were fermented side by side.

The fermentation did not start so soon as in the other wine experiments, the large percentage of glucose having delayed the development of the yeast cells nearly two days beyond the usual time. On the tenth day 10·62 per cent. of alcohol had been generated in the juice, and at the end of a month the percentage of alcohol had increased to 17·26 per cent., being 1·39 per cent. less than the quantity produced in the sample which contained the lower percentage of glucose.

It is upon this and similar results obtained under the like circumstances that the statement above is founded, *viz.* that the alcohol and sugar combine to act as an antiseptic.

In all these cases the wine ferment has the advantage, and has proved itself to possess greater fermentative powers than the malt ferment.

From the results obtained in the fermentation of grape juice I cannot refrain from expressing my opinion that it would be an improvement in the manufacture of wines if, where the percentage of glucose in grape juice is low, an addition of grape sugar were made before or during the fermentation of the "must."

The addition of the sugar would assist in exhausting the juice of its fermentative power which is generally in excess of the glucose naturally present, and tend to prevent acetous fermentation, and the development of obnoxious fungoid growths. It would also have the effect of obviating to a great extent the necessity for fortifying many descriptions of wine, and impart to them a greater body by reducing the proportion of water added and which is present in the alcohol used for fortifying.

In selecting, however, samples of glucose or sugar for this purpose great care would require to be exercised, to ensure their freedom from microscopic animals and germs.

In several instances the fermentation in wine juice was started by the addition of brewers' yeast.

The juice was prepared from foreign red grapes, and the percentage of glucose was made up in one instance to 36 and in another to 31 per cent. To each sample, which consisted of about two quarts, 250 grains of brewers' pressed yeast were added, and the fermentation was conducted in a chamber at a temperature of 70° Fahr. On the third day a small quantity of fresh husks of English hot-house grapes with pulpy matter attached was added to each sample, to ensure, as was thought, sufficient fermentative power.

At the end of fourteen days the samples contained respectively 13·23 and 15·91 per cent. of alcohol, and at the end of a month the percentages were 13·76 and 15·96, being only an increase during the interval of ·53 in the former case, and ·05 in the latter, so that it was obvious the fermentation in both was completely suspended.

The wine was then carefully drawn off, and a sugar solution was added to each residue, and the flasks were replaced in the fermenting chamber. In a few days a brisk fermentation commenced in both samples, and at the end of a week they contained 7 and 6·5 per cent. of alcohol respectively, thereby showing that the fermentative power was not exhausted, and that the stoppage of the fermentation was due to some other cause.

Now it must be observed that the fermentation had proceeded to a considerable extent before the yeast cells natural to the grape juice had time to develop, and there is no doubt that the alcohol previously generated seriously affected their growth and propagation.

An experiment which was made on another sample will perhaps tend to explain this.

A wine juice made up to 36 per cent. of glucose, and from which the greater part of the husks and pulpy matter had been separated, ceased to ferment when the alcohol had reached 15·97 per cent. With a view of trying to push the fermentation further, a quantity of fresh husks and pulpy matter was added, which had the effect of reducing the proportion of alcohol present to 13·93 per cent. The sample was then successively tried in a chamber kept at a temperature of 68° and 80° Fahr., but no further fermentation could be induced, a result which would appear to indicate that to obtain a good and an exhaustive fermentation the cells require to develop naturally in the juice, and become gradually acclimatized to the successive changes of conditions produced during the progress of the fermentation.

Effect of Change of Soil.—A few experiments have been made

to ascertain the influence of change of soil upon the development of the yeast plant.

To a solution containing 32·24 per cent. of glucose some albumen of egg and a small quantity of healthy wine ferment were added, and the sample was placed in a chamber maintained at a temperature of about 67° Fahr.

The wine yeast cells remained bright and healthy, but did not appear to possess much activity; in the course of seven or eight days albumen cells were developed, and there was an apparent action going on in the liquid.

At the expiration of nineteen days the sample was examined for alcohol, and it was found to contain only ·71 per cent., and at the end of a month it was again examined and found to contain ·92 per cent. and one-tenth per cent. of acid.

It was obvious from this result that the soil was unsuited for the growth and propagation of the wine yeast cells. The sample was now diluted by the addition of water until the percentage of glucose was reduced to ·18 per cent. This change of condition, however, had little influence in promoting the action of the cells, and the percentage of alcohol at no time afterwards was found to exceed 1·04 per cent.

At the end of several weeks a new form of fungoid growth was found to have been developed in the liquid and a great mass of fresh cells produced, whose action appeared to have completely superseded the alcoholic ferment, and when the sample was next examined it was found to contain no alcohol whatever, but 2·80 per cent. of acid.

Two malt extracts were prepared, and the glucose present was made up in each case to about 30 per cent. To one sample wine ferment was added, and to the other malt ferment. The experiments were conducted, so far as was practicable, under the like conditions; but the results were somewhat different, the proportion of alcohol produced in the sample to which the malt ferment had been added having exceeded the quantity produced in the other; and it may be added that the spirits produced in the samples were perceptibly different in flavour.

Although this experiment is not regarded as conclusive, still it is worth recording, as the result tends to the same conclusion as that indicated by the results obtained in some of the other experiments, in showing that the different ferments have their favourite soils, and that they flourish best in those natural to them.

The results of the experiments would probably bear a more general conclusion in indicating a comparative inactivity of fungoid spores, unless when they meet with a soil that is favourable to their growth and development.

This paper, it will be noticed, extends over a wide field, and there are many points which require further elucidation. With this view additional experiments are being made, and I hope shortly to be able to go more fully into some of the subjects that are here but lightly touched upon.

EXPLANATION OF PLATE LV.

FIG. 1.—Cells from egg albumen in sugar solution.
,, 2.—Cells from gluten in sugar solution.
,, 3 and 4.—Cells and mycelium from flour and malt albumen in sugar solutions.
,, 5.—Yeast cells developing in mucilage in barley meal extract.
,, 6.—Mould, &c., from malt extract in sugar solution.
,, 7.—Mould from lemon juice in sugar solution.
,, 8.—Mycelium and cells from pus in sugar solution.
,, 9.—Exhausted cells from wort ferment.
,, 10.—Exhausted cells from wine ferment.

II.—*The Origin of the Colouring Matter in Mr. Sheppard's Dichroic Fluid.* By E. RAY LANKESTER, B.A., F.R.M.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *June* 8, 1870.)

MR. PRESIDENT,—Since you have lately twice publicly alluded to the views I expressed on the above matter in 1867, and have declared that the recent observations of my friend Mr. Sorby have entirely "disestablished" those views, I beg to forward you these few lines relative to the matter, and to assure you that, so far from being in the position of the Irish Church, the opinion which I held is entirely confirmed by what has since transpired.

Mr. Sheppard's paper, published in the 'Quarterly Journal of Microscopical Science,' 1867, p. 64, was entitled "On an Example of the Production of a Colour possessing remarkable Properties by the Action of Monads (or some other microscopic organism) upon Organized Substances," and he distinctly advocates therein (as did you also, I believe) the view that the dichroic fluid was due to the conversion of albumen artificially added or naturally present by the working of some monads or other. He refers to Pasteur's observation of the action of monads in changing the colour of certain bodies, and no one can deny for a moment that this was his and your view of the origin of the colour.

It was this method of looking at the matter which I declared to be "unnecessarily conjuring up a mystery," to which opinion I still most strongly adhere. At the same time I acknowledged in my note in the 'Quarterly Journal of Microscopical Science,' 1867, p. 284, the great interest of Mr. Sheppard's observations, and I am glad to be able again to say that we are indebted to him in

England for drawing so many persons' attention to the subject. In my letter, which you have considered to be disestablished by Mr. Sorby's paper, I express the opinion that Mr. Sheppard's coloured fluid probably owes its colour to Cohn's Phycocyan, or a closely-allied body, and that opinion is shared by Mr. Sorby and everyone else with whom I have conversed who has looked into the question, and I am unable to see in what respect Mr. Sorby's observations confute my views, since they are almost identical with his own. Mr. Sheppard declared that his coloured fluid could not have resulted from the algæ which were present, most emphatically. *Now* I imagine there is no one who will not admit that it does result from decaying algæ (the colour existing in living algæ at first), which I stated in the letter supposed to be disestablished, and which Mr. Sorby fully endorses in his paper, speaking, as he does, of the confervoid growth "decomposed with water" and "decomposed with albumen."

The fact is, Sir, that, word for word, I am prepared to repeat the letter published in 1867, and beg your readers to turn to it in order to spare your space. They will find that, so far from being disestablished, the view which I expressed—which was the simple, straightforward one—is the view which everybody takes—probably Mr. Sheppard himself—at the present time.

There are two important facts indicated in Mr. Sorby's paper which have nothing whatever to do with the above matter, but which I will here mention. He endeavours to show that from Mr. Sheppard's two-banded fluorescent fluid—which I am inclined to consider identical with Cohn's Phycocyan, but which Mr. Sorby, not having seen Cohn's paper, does not feel sure about—a single-banded blue and a single-banded red fluid may be obtained. He succeeds in obtaining the red fluid from the two-banded fluid of Mr. Sheppard by the action of absolute alcohol. The single-banded blue fluid he has obtained from fresh specimens of confervoid growth "decomposed with water." The separation of these two colours is important, but it does not in the least affect their relation to the Phycocyan of Cohn, for the spectrum of Phycocyan as figured by Cohn is two-banded (except a very faint line at 9), and agrees with that of Mr. Sheppard's fluid, if we make allowance for strength of solution and difference of spectroscopes. The second point which Mr. Sorby makes out is an action of albumen in promoting the formation of the second constituent of Mr. Sheppard's solution. This is very different from the action which Mr. Sheppard supposed he had made out, namely, that the albumen was converted into a colouring matter by monads. Mr. Sorby seems to think that the albumen really takes part in promoting a chemical change of the cell contents or of the colouring matter of the Oscillariæ. If it does, its action is merely "catalytic," as chemists say; but I believe that it

simply causes a more rapid solution to be obtained by its affinity for the colouring matter in the still living algæ, or by starting the decomposition of the Oscillaria favours the solution of this part of its pigment. Any such action of albumen would be very different from Mr. Sheppard's monad-mystery.

It is worth noting that Cohn speaks of a red modification of his Phycocyan (which is usually blue in solution with carmine fluorescence), not to be confounded with his Phycoerythrin, which gives an exact parallel to Mr. Sorby's observation of a blue and a red condition of the fluid from such confervoid growths as Mr. Sheppard's.

The discussion about the origin of this colour is really almost ludicrous, and very nearly as devoid of justification as would be a discussion as to the origin of the yellow colour when saffron is put into water. The drawings of Mr. Sheppard's confervoid growth which you were so kind as to send for my inspection, prove it to be one of the true Oscillariæ as defined by Kutzing. Some of a growth which Mr. Sorby has had the great kindness to send me from Derbyshire belongs to the same species. Now it is an established fact that these algæ, as well as certain lichens and fungi, give colouring matters soluble in water after the death of the plant, and often fluorescent. It is a serious obstacle to the progress of knowledge when the work of previous observers is ignored, and wild speculation is used instead of the comparison of fact with fact. The paper of Cohn in Max Schultze's 'Archiv,' 1867, Part I., must be looked at by those who wish to discuss this question; also that of Dr. Askenasy in the 'Botanische Zeitung,' July 1867, which is a most interesting memoir, giving intensity diagrams of the spectra of—1, a colour from Oscillariæ, allied to or perhaps the same as Mr. Sheppard's, and as Cohn's Phycocyan, though there is discrepancy in the figures given by the three authors of the spectrum, which is easily accounted for; 2, of a similar colour from the lichen *Peltigera canina;* 3, of another similar colour from *Collema.* The colouring matter known as Cudbear, obtained from a lichen, may be also studied in comparison with the colour from Oscillariæ.

It is a mistake to suppose that the mats of Oscillarian growth are dead because they have such a powerful and unpleasant smell. It is not until they are dead that the cells of the plant allow the colouring matter to pass into the water, but it is easily to be recognized in the living fronds. Its presence in the water is the result of the decay of the algæ, but it is formed in the living plant.

I am able to give a very complete answer to the theories as to the formation of the colour outside the plant and after its death (which are, indeed, inconsistent with all analogy) by the study of some of the growth—identical with Mr. Sheppard's—for which I am indebted to Mr. Sorby's kindness. The two bands described

by Mr. Sorby in the May number of this Journal, and by Mr. Browning in 1867, I have observed clearly *in the living plant*, accompanied by the strong line of chlorophyl in the red, which also forms part of the plant's colour; and of the two lines of Phycocyan, that at $4\frac{1}{8}$, in the green (characteristic of Mr. Sorby's red constituent), was the stronger.

Cohn describes his Phycocyan as being the blue colouring matter of all the Phycochromaceæ, which includes many genera besides Oscillaria. It is possible that among these genera there may be other colouring matters, but it is not impossible that variations are caused by the preponderance of either the red or blue element of Sorby, and this preponderance may vary with the season or other conditions. The spectra from Peltigera, Collema, and Oscillaria, given by Askenasy, are all much alike, and differ chiefly in the relative intensities of the two bands; Mr. Sheppard's fluid and Cohn's Phycocyan are equally close, but the colouring matters of Peltigera and Collema are likely enough distinct from that of Oscillariæ, though this has to be proved. The want of some universal standard for indicating spectra is strongly seen in the difficulty there is in comparing the statements of these authors. The Phycocyan of Cohn, and the colour described by Sheppard, Browning, and Sorby, are so similar in properties and mode of occurrence that it appears probable that the small differences in their spectra, as figured by Cohn and Browning, may be explained away. Whether identical or not, they are so closely allied that it will be well to speak of Mr. Sheppard's as the Phycocyan of Sheppard, and Cohn's as the Phycocyan of Cohn; it being understood that, as shown by Sorby, a red and a blue constituent may be distinguished in these.

I trust I have not made this communication too long, the purpose of it being to remove the misapprehension to which your remarks in the annual address relative to Mr. Sorby's views and my own might give rise, and I hope that you will now reconsider my ecclesiastical deposition, and if not endow, at any rate re-establish the doctrine of

Yours very truly,

E. Ray Lankester.

Note.—The President, in forwarding this letter for insertion, desires us to state that the establishment of the truth will leave no room for Mr. Lankester's assertion that Mr. Sheppard has "quite unnecessarily conjured up a mystery." An extract from Mr. Glaisher's Presidential Address, published in April, 1868, gives Dr. Cohn's view of the mystery in question:—"Dr. Cohn, in thanking Mr. Sheppard 'for his highly-interesting communication,' admits the necessity of further experiments, 'that the truth may be established;' and after intimating his intention to pursue the subject further, he concludes, 'I shall also endeavour to repeat your experiments with albumen, the influence of which upon the colour seems very curious after your investigations.'"—Ed. M. M. J.

III.—*Object-glasses and their Definition.*

By F. H. Wenham, Vice-President R.M.S.

As this subject is now under discussion, I again venture to trespass on these pages with a few remarks. If I incline to defend our highest powers against the assertion that "the best of them produce spherical aberrations," it is not because I am disposed to let well alone, and rest under the personality of a "satisfied optician," but on the plea that both the assumed bead structure of the *Podura*, and also the optical means resorted to for demonstrating such an error, have failed in proving its existence.

By the use of the plane mirror and sunlight, Colonel Dr. Woodward at once cleverly hits upon the illumination that affords an analysis of this beaded appearance, and considers it as an interference phenomenon. That this gentleman possesses object-glasses of the first quality, with the most unrivalled skill in adjusting and using them, cannot be questioned, as his superb photographs of difficult test-objects testify. Having seen the beaded appearance, his extreme diffidence about obtaining a photograph of it is not very encouraging for the advocates of this structure, and Dr. W. suggests that Dr. Maddox be induced to undertake the task. If this structure is to be generally admitted it will be by the aid of the skill of these gentlemen, but I much doubt whether it can be accomplished, for there is a truthfulness in the photographs of these tests that scarcely admits of a deception. The most difficult markings will come out, almost in spite of the illumination, which does not even require that nicety of adjustment sometimes so tedious in ordinary observation.

In November, 1854, I read a paper "On Microscopic Photography" before the Microscopical Society. I had previously obtained impressions of several test-objects, and exhibited one of the *Angulatum* magnified 15,000 diameters, and stated at the time that it "shows the configuration of the markings perfectly black and distinct, in a far greater degree than we can ever hope to see them through the compound microscope, and it is my opinion that if ever the structure of these difficult tests is to be proved it will be by the aid of photography." This prediction has since been verified by the labours of Drs. Maddox and Woodward.

The assertion that "if a blaze of light is sent through the microscope, false appearances are exhibited," must need some qualification. I have condensed the rays from the clear meridian sun, first with a bull's-eye three inches in diameter, and then through an achromatic condenser, upon the *Rhomboides* and other difficult tests, which are thus enveloped in a fearful blaze of light and heat, but gloriously does the picture emerge at last with the most delicate markings shown on a screen 10 feet distant. This is unquestion-

ably a test for object-glasses of good quality and workmanship, for not only may chromatic and spherical errors be detected, but any scratches or particles of dust on the surface of the lenses may be mapped out.

It was remarked by Mr. Slack (who may be presumed to be familiar with the illumination employed), "that the effect produced by Dr. Pigott had no connection whatever with the mode of illumination adopted by Dr. Woodward." As the question at present stands, such a defence is easy, for I in common with many others may well reiterate the question, What is Dr. Pigott's method of illumination? The directions given in the paper of November 10th, 1869, are so vague and scanty, that many having the best glasses are unable to develop the beaded structure, and those that do obtain it are compelled to find out a method for themselves as Dr. Woodward has done.

It is scarcely to be expected that those who have not in some degree been practically familiar with the construction of object-glasses, can be fully aware of the value of the mercury globule in originating combinations. To the optician it is as needful as the callipers and straight-edge to the engine-fitter—every glass is separately tested by it. Its familiar readings show whether the work is going on right or wrong; by the indications of inward or outward coma whether the oblique pencils are correct, and finally, the least chromatic or spherical error can be ascertained by its means. It may be "well-known to mathematicians that these globules are not perfectly spherical" (and mathematicians will be correct in all things), but setting aside the fact that the more minute the particle the nearer it approaches to a true sphere, it happens that shape is not of the smallest consequence, or whether it is illuminated by oblique light, for it is not the *globule* but the absolute point of light reflected from it that is used. The diameter of a mercury globule for correcting the highest powers from a $\frac{1}{8}$ upwards is only the *one five thousandth part of an inch.* Perhaps some one who thinks it may advance the subject, will be good enough to calculate the size of the image of a small lamp-flame set at 4 inches distance, reflected from the surface of a convex mirror of $\frac{1}{10,000}$ of an inch radius.

Dr. Pigott, by converting the microscope object-glass into a species of telescope, and viewing distant and minute discs of light, professes by means of the "Aplanatic Searcher"!!! to have discovered spherical error in all our best glasses, to the existence of which everyone else has hitherto been blind. Doubtless a very imposing or "striking" demonstration may be made out of this, but it is easy to demonstrate that by so doing we are setting at naught the very qualities and advantages of large angular pencils. The conjugate foci are now so far distant, that large angular aperture no longer exists. A difference in the adjusting collar that would pro-

duce an enormous amount of spherical aberration, when the object-glass is tried on the globule test, or in its legitimate use as a microscope lens, is scarcely perceptible in the telescope arrangement, and though a badly-corrected glass may not form an image, yet I have no hesitation in affirming that a lens may be made to give perfect definition under the latter condition, that will prove utterly worthless as an objective for the microscope.

IV.—*On the Optical Advantages of Immersion Lenses and the Use of Deviation Tables for Optical Research.* By Royston-Pigott, M.A., M.D., M.R.C.P., F.C.P.S., F.R.A.S., late Fellow of St. Peter's College, Cambridge.

Introduction.

One might almost venture to declare that perhaps too much exclusive attention has been paid to objectives to the neglect of the pencils radiating from the object. A brilliant particle, as a podura or diatom- spherule, throws out a divergent pencil, which suffers extraordinary deviations and reflexions before it is permitted to enter the object-glass at all. The infinitely small here escapes our attention. But the primary behaviour of the tiny spray of rays is of the last importance to the final definition, according to the refractions, dispersions and reflexions it undergoes in its primary or *nascent state*, if the term be allowed, of the diverging pencils. Again, immersed in a refracting fluid and again covered, new transformations into fresh strands of the divergent pencils impose new optical conditions. A large proportion of the rays are absolutely refused admittance by the covering glass. But some of these rejected rays are at once permitted to pass into the objective by the intervention of fluid-films instead of air.

The tracing of the rays (to scale) for many different selected divergent rays emanating from the brilliant particle, and the investigation of their behaviour under the different resistances offered by interposed media of different deflecting powers, is a problem worthy of a great deal of hard labour. The right understanding of this primary process, among the innumerable rays just about to enter the object-glass, nascent as it were from their very source of light, *may possibly lead to some new and beautiful results at present unexpected.*

The student of these nascent rays, however, must be prepared for some laborious work at the logarithmic tables of numbers, and sines of the divergent angles, and calculate accurately the mutual refractive indices between the different substances and each other.

At present, after much opposition in this country, the method (so much adopted abroad, in Europe and America) of immersion lenses has gained a remarkable footing, water being employed between the objective and the covering glass.

I propose in the following papers to give an account of some researches in this difficult subject, which I hope may tend to stimulate others to more successful exertions than my own.

And to introduce the reader at once to the subject-matter, I will just mention here some results already obtained, which seem to repay the trouble of making the tables.

The greatest aperture of a pencil divergent from a brilliant particle immersed in Canada balsam and covered with a thin plate-glass cover, is only

$$2 \times 41^\circ 48' \quad \text{or} \quad 93^\circ 36'.$$

When water is substituted for air this is enlarged to

$$2 \times 62^\circ 57' \quad \text{or} \quad 125^\circ 54'.$$

If a flint-glass cover could be made, the divergent pencil admitted to the object-glass when oil of turpentine is the immersion fluid would mount up to

$$2 \times 78^\circ 37' \quad \text{or} \quad 157^\circ 14'.$$

Another point of most essential importance is the actual *deviation of the pencils*. The great desideratum is a minimum deviation for all pencils, and therefore a greater simplification of the refractions. On this point, to tempt the reader to attentively consider the subject, I will venture to assure him that the quantity of light and the chromatic dispersion of the divergent pencils of the object is wondrously changed according to the optical nature of the film—be it in water or other fluid—interposed between the objective and the covering glass, though the thickness of this delicate plate may not exceed half a hundredth of an inch. Thus it will be found that calling the quantity of light gained by a given dry or pneumo-objective *unity*, that for other arrangements it may be thus arranged :—

	Quantity of Light.
Dry objective	1
Water lens..	$2\frac{1}{4}$
Oil of turpentine	$3\frac{1}{2}$

Once more; I am about to discuss the amount of deviation for different media from 1° to 90° of incidence, occupying this minute yet interesting interval. That the deviation for moderate angles is *three times less for the water lens than for the dry* is a novel finding which should stimulate inquiry. It is probable that the means really at our disposal for improving even our best glasses are very far from being exhausted. Let not our motto be, "It is enough, rest and

be thankful," but let us prosecute the search for higher achievements still.

Reasons for the Construction of the Tables.

It often happens that even opticians, as well as amateurs, are at a loss to draw accurately on a large scale (necessary for useful inquiry) the path of a ray of light traversing successively media in contact, possessing different refractive properties and densities, and at different angles of incidence measured from the normal to the surface at the points of penetration or emergence. Frequently the possession of such tables would have greatly assisted the writer in original research; and he therefore supposes others would find them equally useful. They require to be calculated for refractions from one substance into another, and not merely for a ray passing from air into a more refracting medium.

Thus though the indices of refraction are for

	Mean Rays of the Solar Spectrum.
Plate glass	$\mu = 1 \cdot 500$
Canada balsam	$\mu = 1 \cdot 532$
Water	$\mu = 1 \cdot 336$
&c.	&c.

the path of the ray of light cannot be shown on paper very readily, except as passing from air into the substance whose refractive index is known, and then only by taking the sines of the angles of incidence and refraction, as $\mu : 1$. But the practical difficulty is greatly increased when the ray is passing from one medium into another of considerable deflecting power, as from glass into water; for the *mutual* or intermediate index of refraction is then considerably modified. Having carefully calculated these quantities, I may state that the actual index of refraction for a ray of light passing

From	Canada balsam	into	plate glass	is $\frac{1}{0 \cdot 9791}$
"	Crown glass	"	flint	" $\frac{1}{0 \cdot 83705}$
"	Canada balsam	"	flint glass	" $\frac{1}{0 \cdot 07954}$
"	Turpentine	"	"	" $\frac{1}{0 \cdot 98033}$

The deviations of the ray entirely depend upon these quantities peculiar to the refraction between the contiguous media. For example—

Suppose a brilliant particle be immersed in Canada balsam, and that a ray of light emanating from it traverses successively, the balsam, the glass cover, a layer of water, and then one of glass (the front lens of the objective), are there any optical advantages gained

by the use of this immersion lens? Mr. Ross confidently advertises his immersion lenses as giving most brilliant definition, and with perfect good faith. And their superior action in many cases testifies most strongly to the inferiority of the dry objectives. The superb definition of Messrs. Powell and Lealand's newly-contrived immersion lenses leaves little to be desired. But those eminent makers acknowledge that their best dry objectives are inferior to their immersion glasses. There must therefore be a cause of this inferiority. A study of deviation, especially when a minimum, may possibly account for some of the causes of this fact, now well accredited with advanced microscopists, and proved by the *enumeration* (not merely by the resolution) of Nobert's XIXth band by Powell's $\frac{1}{16}$ *immersion alone.* The dry lens would not do it in Dr. Woodward's hands.

The method of obtaining the ratio of the lines of refraction and incidence between two media whose refractions are known out of air, can be obtained by dividing the one index by the other.

Thus suppose it be required to find the refractive index between plate glass and water, *i.e.* between μ_G and μ_W.

$$\text{Refractive index required} = \mu_G \div \mu_W$$
$$\text{,, \quad ,,} = 1 \cdot 500 \div 1 \cdot 336$$
$$\text{,, \quad ,,} = 1 \cdot 1227 = \frac{1}{0 \cdot 890666}$$

Suppose, again, a ray of light emanating from a brilliant bead immersed in Canada balsam penetrates it at an obliquity of 43°; if now there is air on the other side of the cover, the ray will be entirely *turned back* by internal total reflexion, and it cannot reach the objective or the observer's eye at all. But now insert a drop of water, and it will then only deviate just about seven degrees, and attain an obliquity of 50° in the film of water. Again, every ray of greater obliquity, up to 62° 57′, will be able to reach the facet lens of the objective *viâ water*, whilst only those less oblique than an angle of 41° 48′ can possibly reach the objective *viâ* air.

One advantage, then, of water versus air is that it enables the objective to gather oblique rays, striking it *viâ water* which could not reach it *viâ air*.

Another advantage will be seen directly, that up to 100° objective aperture the *deviation* caused by the water is three times less than that caused by air; and therefore eccentrical aberration is correspondingly diminished. The immersion lens, therefore, prevents the escape of more than half of the rays diverging from a podura bead if mounted in Canada balsam, so that the quantity of rays transmitted by the water is two and one-fourth greater than that by the dry lens, as will be shown.

But it will be seen hereafter that the water itself introduces a new compensation for spherical aberration. To avoid the analytic calculations the fact will be geometrically displayed in the drawing accompanying the second part of this paper, where every refracted line has been laid down by calculation, and the angles assiduously protracted by scale.

The course of each ray is given by the following table for refraction from plate glass into air, and the mode of calculating the tables is indicated by two examples in the appendix.

TABLE I.

Refractions between Air and Glass. (Air.)		Obliquity ϕ of Ray entering Objective.	Refractions for Immersion. (Water.)	
ϕ'	Deviation.		Deviation.	ϕ'
° ′	° ′	°	° ′	° ′
0 40	0 20	1	0 7	0 53
1 20	0 40	2	0 14	1 46
2 00	1 00	3	0 20	2 40
2 43	1 17	4	0 26	3 34
3 20	2 40	5	0 33	4 27
6 41	3 19	10	1 6	8 54
9 56	5 4	15	1 40	13 20
13 7	6 53	20	2 16	17 44
16 22	8 38	25	2 53	22 7
19 45	10 15	30	3 33	26 27
25 25	14 35	40	5 4	34 56
30 43	21 54	50	6 59	43 1
35 16	24 44	60	9 31	50 29
38 47	31 13	70	13 15	56 45
41 2	38 58	80	22 20	57 40
41 37	43 23	85	23 42	61 18
41 48	48 12	90	27 3	62 57
Angle of Refraction.	Deviation.	Inclination of Ray to Axis.	Deviation.	Angle of Refraction.

The practical importance of "Tables of Deviation" for compound refractions will be acknowledged at once by persons accustomed to original research in optical science. Their calculation, as will be seen in the appendix, involves considerable expenditure of time; but if these tables should in any way advance microscopical perfection, and enable us to clear away the errors still *easily detected* in most of the objectives now in use, the writer will be amply rewarded for his pains.

Apparently deviations for every degree up to 5°, and then for increments of 5° and 10° are sufficiently close for practical use. The refractions are in each case actually taken to the nearest minute.

By these tables the ingenious amateur and optician will at once be able to delineate the course of a pencil of rays through a variety of compound refracting media with precision and confidence.

In the first table comparative deviations and refractions, and the angles of total internal reflexion beyond which obliquity no ray can pass, corresponding to given angles of obliquity of a ray just about to enter the front lens of the objective, have been carefully calculated and verified.

The semi-aperture of an objective being given, *the middle column* representing that aperture at once gives on inspection, in the left and right columns, the deviations which the ray had suffered in entering *air* or *water*, and the diverging of the pencil radiating within the slide (mounted with Canada balsam) from the observed microscopical particle supposed to be brilliant with illumination. If the action of the Canada balsam be required in addition, which is neglected in this table, the deviation will require a *minus* correction exactly given in Table III., which is *one minute* for each degree of incidence or obliquity up to $10°$ nearly, and so on.

The table requires but little explanation. It will be at once seen on inspection that a ray of light up to $50°$ obliquity in water, just entering the objective, suffers only one-third the deviation that a ray of the same obliquity would have undergone through air.

Again, a pencil of $35° 16'$ emanating or diverging from an observed particle in Canada balsam will enter the objective through a film of air at an obliquity of $60°$, and therefore *cannot be transmitted, unless the objective possess an aperture of* $2 \times 60°$, or $128°$; whilst the same *aperture* of objective will, with the optical advantage of water immersion, transmit an oblique pencil radiating from the brilliant particle of $50° 29'$ instead of $35° 16'$ as with the dry lens.

This example at once demonstrates that the addition of a water film is equivalent to largely increasing the aperture. So that *viâ water* a given objective will admit with a small aperture as large a radiant pencil diverging from the observed particle as a much greater aperture-objective will admit with the dry lens.

In other words, when we use an immersion system we can immediately reduce the aperture, *i. e. reduce eccentrical aberration*, and secure less deviation (one-third nearly), and the mystery of the superiority of the water lens is at once laid bare by a study of this table. The relative semi-apertures of dry and immersion objectives to admit identical divergent pencils from this particle are therefore found in the extreme left and right hand columns marked ϕ' in each case; ϕ being the angle of incidence on the objective. So that in each case

$$\text{Sin. } \phi = \mu \text{ sin. } \phi'. \quad (\mu, \phi \text{ and } \phi', \text{ being general.})$$

Thus by the table the apertures of dry and immersion lenses transmitting the same pencil from the object mounted in balsam are, doubling the semi-apertures (for water films),

Apertures. Dry Objectives.	Equivalent to (Nascent Pencil.)	Apertures of Immersion Objectives.
°	° ′	° ′
50	32 44	36 40
80	50 50	58
120	70 32	80
170	83 14	96 (nearly.)

Considering, therefore, the enormous difficulties encountered in absolutely correcting the oblique and eccentrical pencils of an objective, and how much their defects are diminished by reducing the aperture, the extraordinary difference between the immersion and dry objective as regards the aperture required to transmit the same actual divergent pencil radiating from the particle in the balsam, we need not be astonished at the comparative *ease* and cheapness with which foreign opticians construct really good immersion water-lenses.

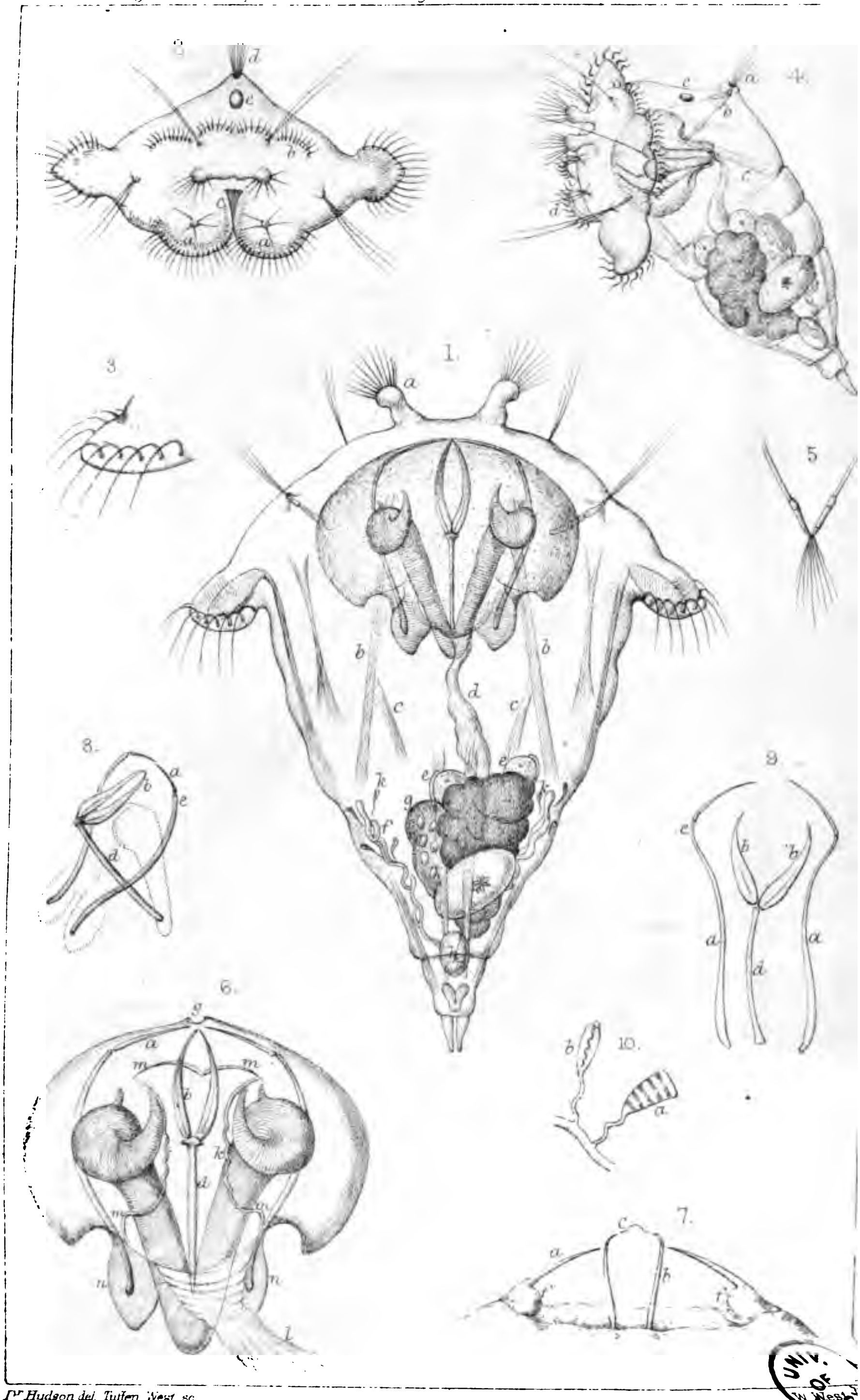
(*To be continued.*)

V.—*On Synchæta Mordax.* By C. T. Hudson, LL.D.

Plate LVI.

Three years ago I found *S. mordax* swarming at Christmas time in a pond near Exmouth on the road to Budleigh Salterton, and though I have frequently since met with specimens I have never till lately found it in sufficient numbers to make it worth while to renew my attempt to make out its structure. However, in the beginning of this March I captured some in a clear pond at Portbury, and by April they had so multiplied in the pond that every dip of a two-ounce bottle would bring up a score of them. They were not to be found everywhere in the pond, but kept mainly to one spot which I noticed was sheltered by hillocks from the prevalent west wind, and in which the water crowfoot was growing. Throughout April they swarmed, but since then their number has been steadily decreasing; and now it requires an hour's search to catch a dozen, the pond all the while remaining apparently unaltered. I had hoped, both here and at Exmouth, to have found the males under such favourable circumstances; but I was disappointed. If I had caught one I think I should have noticed it, as I know the male of *S. tremula.*

Synchæta has had but scant justice done it. All the drawings I have seen (except Gosse's of the mastax) give only the vaguest hints of its internal structure, and represent it from only one (and that invariably the same) point of view; whereas it should be drawn from at least three points of view to convey an accurate idea of its shape. The difficulty of obtaining under the microscope any view of *Synchæta* except that usually given is very great. It is the swiftest swimmer for its size among the *Hydatinea*, as well as the



Dr Hudson del. Tuffen West. sc.

Synchæta Mordax.

most restless among even that restless family. Leydig complains of the incessant activity of *H. senta*—but *Hydatina* has flashes of repose, while *Synchæta* is perpetual motion itself.

When it is swimming in ample space it turns endless somersets in a track regularly curved just like a corkscrew, varying this occasionally by swaying in semicircles from side to side like a skater: more rarely still it will lay hold of something with the forceps of its foot and then spin round its longer axis; and once or twice I have seen it hovering in one spot like a fly over a flower, while its cilia were all the while lashing the water with a fury which rendered them under the dark field of illumination a mere halo round each lobe.

All these varied motions would give the observer excellent opportunities of studying its real shape were it not that they can only be watched in a large glass trough with a low hand magnifier.

When once *Synchæta* is placed between two plates of glass in a drop of water sufficiently small to keep it within the field and tolerably within the focal length even of an inch objective, its characteristic motions cease; it swims incessantly round and across its prison, and at such a rate that the eye gets weary of following it. By gently compressing it one good view can be obtained, that of Fig. 1, but then it is always the same, or at all events can be only slightly altered by reversing the compressorium. In consequence of this no attempt has been hitherto made to describe the top of the head, or to delineate the front or side view. Its outline when swimming with either dorsal or ventral surface at right angles to the line of sight is that of a boy's kite; and exactly where the kite's tassels would be are two movable lobes fringed with a ring of powerful cilia, by means of which the creature performs its various antics in the water. Though the cause of ciliary action seems to be a mystery, yet the various ways in which the cilia of the lobes must work to produce *Synchæta's* motions, admit, I think, of being explained.

But I must first mention how after many abortive attempts I at last succeeded in obtaining a distinct view of the way in which the cilia are set on the side lobes as well as of the whole surface of the head. I had first thought that I might avail myself of its habit of occasionally adhering to the glass and then spinning round like a top, and I reduced the water it was in to a drop barely bigger than itself, and then gradually separated the glass plates of the compressorium so as to draw the drop out into a cylinder, hoping that the animal might thus be forced to stand up as it were on its tail and direct its head to the objective. With *S. tremula* this plan did actually once answer, and I saw an oval mouth and three red specks arranged one above and one on either side of the mouth: but with *S. mordax* I failed completely; it never ceased to turn somersets in the cylinder (though barely covered by it), and with such rapidity that observation was hopeless. Next, following a hint of Leydig's, I

dropped some living specimens into different solutions of bi-chromate of potash, and I soon found that the potash required to be diluted extremely in order to kill the creatures with sufficient slowness. In a solution of 1 in 500 of the volumetric solution of bi-chromate of potash they would live for about twenty-four hours, and die very gently and gradually. I had adopted this plan in the hope that the animal would remain naturally extended after death, and that I might then be lucky enough in the compressorium to get it into the desired position; but on looking at the rotifers after about twenty-four hours' immersion, I saw one swimming very slowly and upright; I at once slipped the trough under the microscope and had at last the pleasure of seeing the head which had so long baffled me: Fig. 2 gives this view of *Synchæta*. About ten minutes after I began my examination the animal died, and I have found on repeating the experiment that the rotifers in the solution must be looked at every now and then before the twenty-four hours have expired, as some will live quite an hour or two longer than others, and as all of them, when they have once reached the stage of feebly swimming upright, soon die: moreover, quite half of them perversely die without going through the ceremony at all.

To return to the methods by which the ciliated lobes produce *Synchæta's* motions. Fig. 3 shows one of the lobes with the cilia on it at rest. The cilia strike the water one after another in turn, generally giving a vigorous downward blow and then gently returning to the position they started from. When these motions take place in planes at right angles to the base of the lobe, they will clearly produce an onward movement in the animal in direction of its length. Should the planes in which the cilia move be oblique to the base of the lobe, the effect will be not only to produce an onward movement, but to throw the animal constantly out of the line of its own length in a direction at right angles to those planes; the result would be a corkscrew path such as *Synchæta* habitually swims in; but if when these planes are at right angles to the base of the lobe the cilia were to strike an upward blow as vigorous as the downward one, the creature would be kept slightly oscillating over the same spot, as the two blows would neutralize each other.

It is by a similar process that a fly hovers in the air, as may easily be seen in the common wasp-like flies in our gardens; for on approaching them closely it will be seen that they seem to have four wings (though really possessing only two), owing to the rapidity with which the backward stroke is instantly balanced by a forward one, and the comparatively longer interval of time which follows each pair of strokes.

If the ciliated lobes are looked at from above when the animal is erect, it will be seen that owing to the cilia beating regularly in turn, a series of waves producing a wheel-like appearance runs round

each lobe. When *Synchæta* seizes any support with its pincers it has only to cause these ciliary waves to run *both* from left to right, or *vice versâ* (the cilia at the same time striking a little obliquely in the same direction), to cause its whole body to spin in the opposite direction round its longer axis. Generally the ciliary waves run in opposite directions and so neutralize the tendency that each set has to turn the animal round its own length.

The ciliated lobes are employed solely for the purposes of locomotion, but the ciliated rings (Fig. 2, *a*, *a*) on the ventral surface, and the interrupted row (Fig. 2, *b*) on the dorsal surface, are used to bring food to the neighbourhood of the mouth (Fig. 2, *c*), which I believe is also fringed with fine cilia, though of this I am not quite certain. The ciliated rings (Figs. 2, *a*, and 4, *d*) are mounted on cup-like protuberances, the edges of which can be depressed or erected at the will of the animal, so as to cause vortices on either side of it as well as in front: this is effected by muscles attached to several lobes on which the cups rest.

On the centre of the head are two projections (Fig. 1, *a*) bearing setæ arranged like a fan, and each of the rings (which are omitted in Fig. 1 for the sake of distinctness) surrounds a papilla (Fig. 2) bearing setæ. The use of these contrivances is easily seen in the living animal; for whenever any prey approaches the mouth the "horns" (as Ehrenberg calls them) and the setæ on the papillæ are bent towards each other and over the mouth, so as to prevent the escape of anything that has been brought by the action of the rings to the mouth. This function of the setæ (as Mr. Cubitt has pointed out) is common to all rotifers; and it may be best seen in some of the *Brachionæa*, in which the setæ are habitually arranged in dome-fashion over the buccal funnel.

S. mordax's head carries also four bundles of setæ, which are, I believe, organs of touch. Each is mounted on a short piston to the end of which a muscle is attached, and each can be protruded or partially withdrawn through its own papilla. The hairs in each bundle often move very rapidly, sometimes all curving over in the same direction like a horse's tail, at others all spread out in different directions like a feather-brush; but such motions are I believe involuntary, and are due to the currents caused by the cilia. I have not yet exhausted the tactile apparatus of *Synchæta*. Its dorsal surface rises in a hump which carries on its vertex in a depression another bunch of setæ (Figs. 2, *d*, and 4, *a*). This is really a double bunch (Figs. 5, and 4, *b*); each springing from the same rocket-shaped contrivance which I have figured in *Hydatina*, *Triarthra*, and *S. tremula;* and the long ends of which are probably nerve fibres connecting the setæ with the nervous mass in the head. These organs in *Triarthra longiseta* have been independently observed and figured by Dr. H. Grenacher, who has given a good drawing of the

rotifer in Siebold and Kölliker's Zeitschrift of Zoology for November, 1869. The setæ are best seen by dark field illumination, and when the animal is swimming freely; but of course only momentary glimpses can be caught of them. When *S. mordax* is held in the compressorium it never ceases to struggle forwards, jerking every now and then its head and mastax inwards by means of its longitudinal muscles. After each jerk, as the head regains its old position the dorsal setæ can be seen to slip back to theirs, and the two rocket heads with their corresponding nerve fibres can thus be made out. So violently is the head drawn in that the loop which these setæ form (Fig. 4, *b*) is thrown forward so that the setæ appear to belong to the ventral surface; and it was some time before I could satisfy myself as to whether there were setæ on that surface or not. I expected to find setæ at the root of the tail as they are in *S. tremula;* but I do not think that they exist. *S. mordax* puts this tactile apparatus to good use, for it will (as I have already mentioned) turn somersets with the greatest rapidity and violence between two plates of glass, separated by barely more than its own length, without striking against them. The red eye (Figs. 2 and 4, *e*), if it is an eye, can be of little service for such pranks, as it lies behind the head and sunk at some depth under the dorsal surface. It is crimson by lamplight, but a bluish-purple by daylight; it is a truncated cone with the broader end downwards, and from this end a tapering prolongation passes downwards to the large nerve mass in the head.

The arrangement of *Synchæta's* muscles is very similar to that of *Hydatina senta;* the two large ones (Figs. 1, *b*, and 4, *c*) that withdraw the head are attached to the dorsal cuticle and have branches (Fig. 1, *c*, *c*) attached to the ventral cuticle, so that the united pull may draw the head down nearly in a line with the creature's longer axis; powerful muscles work the side lobes, and others springing from the middle of the animal work the foot and its pincers: transverse muscles divide the body into segments and enable it to regain its shape after it has contracted in direction of its length.

The mastax (Fig. 6) is enormous; it is similar to that of *S. tremula,* which has been figured by Gosse in the 'Philosophical Transactions' of 1856. Fig. 9 shows the harder portions after treatment with caustic potash, and compressed out of their normal positions; *a*, *a*, are the mallei, each with a solitary sharp tooth jointed at *e*; *b*, *b*, are the rami and *d* the fulcrum. The same letters point out the same parts in Fig. 8, which is an ideal side view of them in their normal position, and in Fig. 7 which is the distended aperture of the mastax.

The mallei are often protruded with a swift snap through the mouth, and the rami too are thrust out in front of the frontal outline, and when so extended appear to be connected by some membrane (Fig. 7, *c*): as the mallei regain their position in the mastax,

muscular bulbs (Fig. 7, *f*, *f*) (like gums, as Gosse says) can be seen enveloping the joint (*e*).

From the aperture in the mastax (Fig. 6, *g*) a wide curved tubular sort of passage leads to the œsophagus (Fig. 6, *l*); and on focussing down to about the centre of the mastax the edges of the passage assume the curious shape drawn at *m*, *m*, *m*, *m*, Fig. 6. This outline encloses the area (Fig. 6, *k*) which would be obtained by a plane section of the passage at right angles to the optical axis of the instrument at the focus of the objective: if a slice were cut off a hollow india-rubber ring, so as to expose the interior without cutting through the ring, a similar (though differently shaped) area would be produced. Inside the mastax is a very powerful V shaped muscle. It is striated; and the striæ produce the blue tint which is so often seen with oblique light on diatoms. The muscle embraces the lower end of the fulcrum, and by jerking it up and forward throws out the rami. The mallei are also worked by large muscles (Fig. 6, *n*, *n*) which bend round their lower ends and are attached to the mastax.

Gosse speaks of muscles at the sides of the mastax "lining the globosity of each lateral lobe." I have tried every species of illumination, and every artifice I could think of, but have always failed to see them.

The whole apparatus is calculated to enable *Synchæta* to suddenly swallow large objects; and although I have never had the good fortune to see it, I suspect that *Synchæta* swallows whole, or nearly so, the smaller specimens of *Sphærosira*, of the remains of which apparently its stomach was frequently full.

The rami with their connecting membrane would arrest the globe as it was carried over the mouth by the ciliary currents, and the sharp points of the mallei would pierce it and drag it down.

The œsophagus (Figs. 1, *d*, and 6, *l*) is very long and flexible; usually the food passes through it direct to the stomach; but *S. mordax* has a trick of occasionally throwing the contents of the stomach into the œsophagus, or of dilating it with water. The usual gastric glands are on either side of the stomach (Fig. 1, *e*, *e*), and there is nothing peculiar either in them or in the ovary (*g*), or the contractile vesicle (*h*). The arrangement of the tubes (*f*, *f*) carrying the pulsatile tags (*k*, *k*) is unusual. In most of the free-swimming rotifers the tubes pass up each side of the body from the contractile vesicle to the head; but in *Synchæta mordax* they barely reach half-way, while branches of them, or of something like them, can be traced half across the gastric glands.

To the vexed question of the pulsatile tags, I should like to contribute a suggestion. I quite agree with Mr. Cubitt in doubting the existence of any cilia in them, for in some positions (especially in the case of *Euchlanis*) they present different appearances, as in Fig. 10.

I have repeatedly watched the same tag and seen it, as it was turned over by the animal's motions, put on either guise: in fact, the tag 10, *b*, is evidently 10, *a*, seen sideways, and I cannot imagine a cilium giving rise to the appearances in 10, *a*. Besides, the use of the cilium is said to be to give rise to currents in the tubes towards the contractile vesicle into which they empty themselves. Now how could cilia produce currents in capillary tubes of such extreme minuteness? Let anyone attempt to blow out water from a glass tube tapering to a capillary end, and I think he will doubt of the power of a cilium to set up a current in it. There is, however, among the multitude of guesses about the water vascular system one ascertained fact, and that is, that the contractile vesicle empties itself into the cloaca. Cohn states that he has seen the current produced in the surrounding water after each contraction, and I have myself witnessed it in *Brachionus*, though I have never seen any return current inwards, as Cohn thinks he has.

If the contractile vesicle were filled from without by a return current, the whole apparatus of tags, &c., would be, I think, inexplicable: but suppose for a moment that Cohn might be mistaken and that some valve or construction prevents any return current, it is clear that a partial vacuum would be produced in the vesicle by the close contraction of its walls on themselves, and if the tubes carrying the tags opened (as they are generally supposed to do) into the vesicle, and the tags had *open* ends, then the fluid between the viscera and the cuticle would be constantly drawn through their apertures into the tubes and so into the contractile vesicle; thence to be expelled through the cloaca.

The rush of fluid through the open ends of the tags, whose true shape is, I think, that of a flattened jelly-bag, might cause a quivering like that produced by blowing between two pieces of paper nearly in contact; and this quivering seen in profile would look like a cilium in rapid action with its free end directed from the aperture, for the wavy motion would die away as the fluid passed from the aperture of the tag into the tube.

Should this suggestion prove to be correct, the apparatus would appear to be of a urinary nature. That it can be for the purpose of aërating the fluids of the rotifer's body with fresh water is hard to believe, when the minuteness of the tubes are taken into account, and also the fact that the rotifers are constantly swallowing large quantities of water.

I have only to add that I believe that *Synchæta*, like *Hydatina*, secretes a viscid fluid which exudes from the extremities of its pincers; and that this summer I have met with one or two specimens of *S. mordax*, whose bodies have been rendered quite opaque by vegetable organisms (like the individual corpuscles of *Sphærosira*) floating in the fluid between the viscera and the cuticle.

VI.—*Notes on Diatomaceæ.*

By Professor Arthur Mead Edwards.

(*Continued from p.* 250, *No. XVII.*)

Along with the *Bacillaria* in the brackish water at Hoboken, I found numerous individuals of an *Amphora*, which I have known in this neighbourhood for many years, and which I considered unnamed as yet. To it I have given the provisional name of *A. lanceolata*, on account of the form of its outline. This genus has always been considered an epiphytaceous one; that is to say, one which grows attached to other plants or submerged substances; yet this form was free and in active motion. In fact I think it was one of the most lively diatoms I ever saw. So another smaller species of *Amphora* which is common near here, is always, as far as I have noticed, free. Here we have species appearing both in the free and attached conditions, and this is even more strikingly illustrated in *Schizonema*.

Bacillaria paradoxa is usually set down as the most rapid in motion of the Diatomaceæ, its velocity being recorded by Smith, as he measured it, at over one two hundredth of an inch in a second. This is certainly pretty quick when we consider that the length of the frustule is only ·0025 of an inch. But my experience has been that its velocity varies in every degree from that mentioned to perfect rest; at times some individuals will be in rapid movement, while others are motionless; and also I have remarked that from sunrise to noon seems to be the period during which, under ordinary conditions, the movement is most active, while during the afternoon it is very sluggish, and at night almost *nil*. This *Amphora*, as I saw it at the time mentioned, was moving even more rapidly than I ever saw a *Bacillaria* move, and that with a steady onward progression very different from that of most naviculiform diatoms.

It appears to me that in *Schizonema* and similar genera, which consist of silicious loricated naviculiform frustules enclosed in membranous tubes, as soon as a rupture of the investing membrane takes place, by fracture or tearing asunder, almost immediately a knowledge of the fact is in some way communicated from the point at which the opening occurs to all other points of the tube, as at once the contained frustules which hitherto have been at perfect rest, or, at most, only moving to a very slight extent, and even then in an extremely sluggish manner, become animated in their motion, and the most of them move towards, and attempt to escape from, the opening made. And this evidently does not result, as might at first have been supposed, from any pressure exerted upon them from the closed end of the tube, and which, therefore, only shows itself when

the obstacle in the shape of the investing membrane is suddenly removed. For the motion is the true lively action peculiar to the living individual in the naviculiform Diatomaceæ, and is not in all cases towards the opening made, but often many, or, as in some cases which have come under my observation, most of the frustules begin to move in an opposite direction at first, while at the same time many escape by the opening in the tube, and thereafter assume vigorous motion in the surrounding liquid. Again, usually some of the frustules, being, as at first appears, carried along by the stream constituting the mass of those moving towards the opening, all of a sudden seem to change their minds, or are struck with an idea, if I may so express myself, and here and there will be seen individuals which at once alter the direction of their course and move in exactly the opposite direction, or backwards as we may say. The individual frustules as they escape from the ruptured end of the investing tube and enter the surrounding water, do so with the peculiar trembling and apparently uncertain movement so characteristic of many of these organisms.

It will be well to note that these observations have been mainly made on *Schizonema Grevillei*, a species occurring very commonly in New York harbour, although I have noticed the same thing to happen with other species of the same genus, and, if I am not mistaken in the allied one, *Homœocladia*.

After a time it would seem that the broken end of the tube becomes closed again; perhaps by the deposition of new matter, or it may possibly be by the action of the surrounding water upon the fluid within the tube, if it be of a different composition (which would seem to be extremely doubtful, however), as the frustules no longer attempt to escape, and resume their quiescent state from which they have been startled by the accident of the rupture, or they move over each other up and down with the same irregularity which is commonly the habit of these forms.

I am strongly of opinion that certainly in some of the cases in which I have seen this escape of frustules take place from the investing tube, it has not resulted from any rupture caused by my manipulation, but would seem to be a normal occurrence. In fact, at such times the diatom is taking upon itself the active or free condition by means of which the species is to be distributed. And we must believe that such is the habit of all so-called epiphytaceous forms; otherwise it is not easy to comprehend how the species become so wide spread as many of them are, for we have not at present any authentic notice of the formation of free-swimming spores in this family. It is hard, when making such observations as those I have here recorded, to believe that these organisms are not endowed with sentient capacities, especially when one sees, as I have, a free frustule of such a *Schizonema* apparently attempt to regain a

lodgment within the tube from which it had some time before escaped, by means of repeated dives towards the hitherto open end, which has since become closed. I have observed such struggles continue for a minute or more, but never with the success apparently desired.

Many months since I mentioned at one of the meetings of the Lyceum of Natural History in New York, that I had seen two apparently different genera of diatoms existing within the same investing tube; and now I wish to place that fact upon record, and state one or two more instances of the same mode of growth. During the month of March, 1868, I found in the harbour of New York specimens of *Schizonema Grevillei* in active motion within their investing tubes, but accompanied by a much smaller form possessing a totally different outline from *S. Grevillei*, being blunter at the ends, and with parallel sides on S. V. During the same month, and also in April, I found this mode of occurrence very common, and also *Schizonema Grevillei* and a *Homæocladia* in the same tube, and *Schizonema cruciger* and the small form mentioned above, both in the same tube, and *S. cruciger* and *Grevillei* in the same tube. In all these cases the frustules were in lively motion, passing over each other from one end to the other of the tube. In May of the present year (1869), I found growing in the salt water of the "Mill pond" at Salem, Mass., *Schizonema cruciger* and *Nitzschia closterium*, W. S. (*Ceratoneis closterium*, C. G. E., and *Nitzschiella closterium*, L. R.), both in the same tube. And here it will be necessary to say something in regard to the form I have called *Nitzschia closterium*, as I shall thereby, I hope, be enabled to clear away a little fog of synonyms. Neither Smith, Kützing, nor Rabenhorst describes or figures any species living within a tube like *Schizonema*, the frustules of which have an outline and markings similar to *Nitzschia closterium*, so that it is not likely that they ever saw anything but the free form or condition of this species. However, Ehrenberg figures and describes, under the designation of *Schizonema? Agardhii* (*Die Infusionstheirchen*, 1838, p. 343, T. xx., fig. xvi.), a form agreeing with this, but the structure of the frustule is that of *Nitzschiella* of Rabenhorst, so that the specific name of this species should be *Agardhii*, whatever its genus be decided to be hereafter. For the present, as it is nearest allied to the forms grouped under *Homæocladia*, it had better be placed in that genus, so that the synonomy would stand thus:—

Homæocladia Agardhii, C. G. E. (sp.). Abhand. K. Akad, Berlin. P. 311. 1833.

Ceratoneis closterium, C. G. E. 1840.

Nitzschia closterium, W. S. 1853.

Nitzchiella closterium, L. R. 1864.

What are we to say to such facts as these I record, as well as

that of which I sent an account, and illustrating specimens to the late Dr. Walker-Arnott—and which has been noticed by Mr. F. Kitton, who examined my specimens, in 'Hardwicke's Science Gossip' for May, 1869 *—of the occurrence of what are usually considered two distinct species of *Gomphonema*, viz. *G. capitatum* and *G. constrictum*, both growing upon the same stipes or stalk! But this is not all. Since then I have made gatherings at the same place, and still find the above two forms growing upon the same stalk, and two others of totally different outline which appear also upon the same stipes. So that here we would have four hitherto considered distinct species arising from the same individual. I do not name the two last-mentioned forms, as I am in some doubt with regard to the names that have been applied to them. The question of what is the individual in the Diatomaceæ is again raised by the observance of these facts, as well as those I described in my "Note on a point in the habits of the Diatomaceæ and Desmidiaceæ," read before the Boston Society of Natural History, January 8, 1868, and published in their 'Proceedings.' † The specimens illustrating the remarkable mode of occurrence of the two forms of *Gomphonema* which I sent to Dr. Arnott unfortunately did not arrive until after his death; but, speaking of my having so found them, he wrote to me in the last letter I received from him as follows. I feel that I am justified in publishing this extract, as it is of such importance; and I also know, from what he wrote to me, that he himself would not object to my doing so were he still living.

"Your discovery of *Gomphonema constrictum* and *capitatum* growing on the same stalk is interesting, if you are not deceived. When a *Gomphonema* spore grows on a weed, the stalk (which is merely the external mucus collected at the one end) is formed by the growing frustule. It is not the stalk (or in *Schizonema*, the tube) which produces the frustule, but the frustule which produces the stalk or tube. Then when the frustule self-divides, several are formed, either side by side, or each may project a new stalk; but seldom with much regularity. Now every frustule and valve arising from the same spore *must be precisely alike*, being all formed from the original frustule by repeated self-division; and as self-division merely repeats the same identical form or variety, it is not easy to understand how it is possible to have two varieties of form on the same stipes. If there be no mistake on your part, you will overturn all the present views of the production of new frustules and valves. It is more easy to suppose that a frustule from another stipes had become agglutinated to the stipes. But as you say you have sent some in a bottle, I will examine it with care when it arrives. Every spore may produce a different variety, but it is not easy to understand that the same spore, or stipes, can give rise to

* Vol. v., p. 109. † Vol. xi., p. 361.

different varieties. As for the two species (*G. constrictum* and *capitatum*) I have never been satisfied that they are distinct, and probably *G. herculaneum* is another variety."

For my part, from the mode in which the two new valves are formed within and between the two old ones, when self-division takes place, I can readily understand how a great variation in both outline and sculpture can occur. In this case the two forms have exactly the same sculpture, and the difference between them merely lies in the outline of the valve. From my knowledge of how greatly this character varies in the Diatomaceæ, I, from an early period in my studies, considered these two supposed species to be but forms of one, and this discovery proves that my surmises were correct. At some future time I may have something to say with regard to the genus *Gomphonema*, and what, in my judgment, constitutes a species in it. I am now engaged, and have been for some years, working up several genera, with the express purpose of determining the true lines of specific distinction in them. And I must be permitted to here enter my earnest protest against the custom which has become so wofully common, in England more especially, of manufacturing species where they do not exist.

The labours of such self-supposed students of nature are more than thrown away. Our books become crowded with worthless synonyms, and this branch of biology has, in consequence, fallen into disrepute among scientific observers generally. If those who have the opportunity of securing and examining specimens of Diatomaceæ, would only study them a little more carefully, and if they must publish, do so only after properly maturing their knowledge, we might hope to learn something of the life history of these strange atomies. Better that really new species should for ever remain unnamed, than that such contributions to the literature of the Diatomaceæ, as appear from time to time in foreign journals, should ever see the light. It is a curious fact that almost everyone who becomes possessed of a microscope of sufficiently high magnifying power, at once imagines that he is abundantly armed and equipped, as well as qualified, to attack and overcome the most difficult problems in biology. Hence we find the most startling discoveries put forth by very immature observers of nature, who suppose themselves to be students, but who have really given little time or thought to study. No branch of biology, perhaps, has been more cursed with supposed discoverers of this class than the Diatomaceæ, until a man comes to be appreciated by the number of species he can manufacture. By far the largest number of observers who are attracted to these beautiful and wonderful atomies forget that we have in them presented to us for investigation one of the most puzzling problems in the whole group of phenomena, illustrating that which we call life; but, on the contrary, appear to

consider them as "simple organisms," whose morphology and life history, as well as classification, are therefore proportionally easy of comprehension. I have devoted many years to the earnest study, under varying conditions, of these examples of complex simplicity, and pity it is that others who have not spent so much time over this branch of organic existence should not have been so fortunate as I was in possessing a wise and patient counsellor in the late Dr. Walker-Arnott. I can truly say that had it not been for his invaluable friendly advice, I too would have doubtless ranged myself with the manufacturers of species and synonym accumulators. Often have the kindly words he has written me made me pause ere I, as he pithily remarked, "rushed into print" with supposed discoveries, which I would have been ashamed of thereafter. Dr. Arnott says, "a microscopist looks on everything as subservient to the microscope, and that whatever he sees, and which appears distinct to the eye, he thinks ought to be described or figured as distinct. I am, on the other hand, a naturalist, a botanist in particular, and use the microscope, simple or compound, as a necessary evil, merely to enable my eyes to see better minute structures; but whether these differences amount to specific or generic importance, or are only peculiar forms of one species, is the result of analogy, a mental process which can only be attained by a training in botany in all its branches, for many years." Natural objects, like the Diatomaceæ, which can only be seen after they are magnified several thousand times, and then only under peculiar circumstances of illumination, must be difficult of comprehension, even if their life history were much more simple and more easily studied than it is. I cannot too strongly caution the intending student of this enticing branch against trusting to a few and hasty observations made upon the dead skeleton of the plant. It is only when they are studied in the living state that the Diatomaceæ can be understood, and even then only with difficulty.

But one more abstract from my note book and I must draw these remarks to a close. In the early part of November, 1868, I made a collection of *Colletonema vulgare*, and for some time have been able to keep it alive in a bottle so as to study its peculiarities. And here let me say that many minute forms of both animal and vegetable life which I have been unable to rear otherwise, I have found to flourish in phials with small necks, or those with large ones which have the aperture partly stopped with a loose cover of some kind. It would seem that the gases given off from the human body, and accumulating in dwelling rooms, in which I have kept specimens, are deleterious to these small forms, and the partial closing of the vessel prevents, to a great extent, their entrance. My specimens of *Colletonema* flourished finely and grew considerably. I have been thus enabled to watch them, as I may say, building their

tubes; this species, consisting of naviculiform frustules enclosed and freely swimming about in tubes, after the manner of *Schizonema*. In fact there is nothing to separate these genera, except that the first inhabits fresh water, whilst the latter is an inhabitant of the sea, where it is to be found generally in profusion, covering larger algæ and rocks. The extension of the tube takes place after the following manner. As the frustules increase by the process of subdivision common to all of the Diatomaceæ, of course the two frustules thus formed occupy double the space of one, and as the cell division is continually going on, after a time the tube must become choked with individuals. At this period in their existence they appear to be extremely active, moving with increased rapidity up and down the tube as freely as their crowded condition will permit. Whether the end of the tube is never closed, or opens at certain seasons, I have been unable to determine; at all events it is now found to be open, and the frustules slip over each other until they reach this opening, and one or two will project outside as if prospecting, and will occasionally return within the general envelope. When a frustule thus projects from the open end of the tube, it never, as far as I have seen, rushes onward with the vigorous motion with which it moves within the envelope, but this is doubtless only so when the tube is being lengthened. It can be easily understood that if the species be disseminated by the distribution of perfect frustules, as seems to be most likely, that they must then escape from the tube after the manner I have recorded above as taking place in the allied genus, *Schizonema*. When one or two frustules have projected from the open end of the tube, they often immediately come to a rest just beyond the tube, or do so after moving over each other slowly outside of, but in a line with, the tube. While at rest there appears to form around them a transparent mucous sheath, which, so that it may not fix them in their position, is kept in a tube form by the frustules again moving over each other, and thus, as it were, fashioning and smoothing the inside of the tube. This sheath becomes more and more dense, until it is plainly visible as forming an elongation of the tube, when the frustules again project from the end, and a new portion is added. I have in this way seen a tube grow across the field of the microscope, and the closely-packed frustules extend themselves in single file, each just overlapping those in front and behind it. The membrane constituting the tube, although dense and strong, is somewhat elastic, but not very much so, for I have seen three or four frustules become wedged together by one attempting to pass backwards whilst the others were moving forwards, and at such times the tube does not stretch to accommodate the crowding, but yet is often bent by the force of the moving frustules. In fact this force must be considerable, as is evidenced by the size of the obstacles, as grains of

sand, which a small diatom will move; and in *Colletonema* I have seen the tough tube membrane bent inwards so as almost to collapse by such a crowding as I have mentioned.

As I have mentioned Mr. Kitton's paper in 'Science Gossip,' I must here take the opportunity of saying something in regard to that article, and I feel sure that he will not take amiss what I shall say when he understands the spirit in which it is written.

First, I wish it to be understood that the specimens and the letter accompanying them were sent to Dr. Arnott for his opinion; thereafter I intended to publish the facts treated of myself. However, as Mr. Kitton has made public his opinions on the specimens, I will now give mine; but the fact of its being a private letter of mine from which he quotes, and one never meant to be made public, must explain what I there say. He has considered the "queer form" to be a new *Fragillaria*, and has named it *Crotonensis*. Evidently he does not agree with that portion of my letter which he quotes, when I say: "I am not in favour of naming forms after places or persons, but strongly incline to distinctive and descriptive names." If the form were a new species I should have named it myself; but Dr. Arnott at once said it was likely to be *Fragillaria capucina* var. γ, and such I see Mr. Roper points it out to be, in the July number of the same periodical.

As it may be of interest in connection with this point, I may say that in a previous specimen of the sediment from the Croton water which I had sent him, Dr. Arnott informed me he had found the following species: *Cymatopleura elliptica*, *Navicula trinodis* and *gibberula*, *Surirella craticula*, *Denticula obtusa*, *Epithemia zebrina*, *Cocconeis Thwaitesii*, *Achnanthes ventricosa*, *Cyclotella rotula* and *operculata*, *Orthosira orichalcea*, *Gomphonema tenellum*, and another intermediate between *dichotomum* and *intricatum*, most likely the latter.—*Paper read before the Boston Society of Natural History, February 9th*, 1870.

PROGRESS OF MICROSCOPICAL SCIENCE.

* *The Anatomy of Tunicates.*—Some observations recently made by the President of the Halifax and Nova Scotia Institute of Natural Science appear to bear out the impression of the vertebrate resemblances presented by Tunicates. In describing the anatomy of *Boltenia clavata*, in a paper read before the Institute (April 11), Mr. J. M. Jones gave the following particulars :—Length of sac, 2 in. 2 lines; breadth of ditto, 1 in. 5 lines; length of peduncle or stem, 9 in. On opening the peduncle from sac to base, I found it hollow the entire length, excepting, perhaps, half an inch immediate to the root itself, which is composed entirely of fibrous cords, traversing and interlacing each other, and, in some parts, forming a solid mass, particularly at the base. On careful search, I could discover no communication between this hollow stem and the sac. Traversing what may be termed the dorsal region of the peduncle, I found a fibrous or muscular cord continuous throughout its length, evidently serving the purpose of a vertebral column. It was hollow for about half an inch from the sac, and communicated with its interior. As regards this muscular column, it is probable that it can be stiffened or relaxed at the will of the creature, and that in either case to enable it to bend its sac to the surrounding sea-bottom, or keep erect. The peduncle is wholly ramified by muscular fibre, which evidently gives it the peculiar toughness which characterizes it. The sac also is of a leathery consistence, but more delicate in appearance, although thicker and equally tough.

* *Perfect Eyes in the Fœtal Mole.*—In a paper read before the Royal Society by Mr. R. J. Lee (April 28), the author detailed with much minuteness his researches on the anatomy of the eye of the adult and fœtal mole. Mr. Lee thus modestly concludes :—It must necessarily happen that many interesting observations are made in the course of an investigation like that which has been briefly described, and many minute details might have been added to this account: but it appeared to me to be desirable to limit the details, as far as possible, to those which were sufficient to establish the remarkable physiological fact *that the mole, at the time of birth, is endowed with organs of vision of considerable perfection, while in mature age it is deprived of the means of sight in consequence of certain changes which take place in the base of the skull, terminating in the destruction of the most important structures on which the enjoyment of the sense of sight depends.*

* *Structure of the Mammalian Kidney.*—At the meeting of the Vienna Academy of Sciences (March 10) Herr Hofrath and Prof. Hyrtl presented a memoir on the pelvis of the kidney of mammalian animals and man. It contains a detailed description of the urinary organs (ureters, pelvis, calyces, and papillæ) of three genera of Cetacea, two of Monotremata, three of Marsupialia, two of Solidungula, six of Ruminantia, twelve of Carnivora, four of Insectivora and Cheiroptera,

* The paragraphs marked * have been standing over since our last number.—Ed. M. M. J.

and five of Quadrumana. The section of the memoir treating of the human kidney is a very elaborate one.

* *Dr. Royston-Pigott's Aplanatic Searcher.*—At the meeting of the Royal Society (April 28th), Dr. Royston-Pigott read a long and important paper on the above subject, and on the effects of the aplanatic searcher in improving high-power definition in the microscope. We have not space for a proper abstract of the paper, which, however, will appear at some length in the 'Proceedings.' The following description may, however, be of interest:—The aplanatic searcher described by Dr. Pigott is intended to improve the penetration, amplify magnifying power, intensify definition, and raise the objective somewhat further from its dangerous proximity to the delicate covering-glass indispensable to the observation of objects under very high powers. The inquiry into the practicability of improving the performance of microscopic object-glasses of the very finest known quality was suggested by an accidental resolution in 1862 of the Podura markings into black beads. This led to a search for the cause of defective definition, if any existed. A variety of first-class objectives, from the $\frac{1}{16}$th to the $\frac{1}{4}$th, failed to show the beading, although most carefully constructed by Messrs. Powell and Lealand. Experiments having been instituted on the nature of the errors, it was found that the instrument required a better distribution of power; instead of depending upon the deepest eye-pieces and most powerful objectives hitherto constructed, that better effects could be produced by regulating a more gradual bending or contraction of the eccentrical rays emanating from a brilliant microscopic origin of light. It then appeared that delusive images, which the writer has ventured to name *eidola*,* exist in close proximity to the best focal point (where the least circle of confusion finds its locus). (I.) That these images, possessing extraordinary characters, exist principally above or below the best focal point, according as the objective spherical aberration is positive or negative. (II.) That test-images may be formed of a high order of delicacy and accurate portraiture in *miniature*, by employing an objective of twice the focal depth, or, rather, half the focal length of the observing objective. (III.) That such test-images (which may be obtained conveniently two thousand times less than a known original) are formed (under precautions) with a remarkable freedom from aberration, which appears to be reduced in the miniature to a minimum. (IV.) The beauty or indistinctness with which they are displayed (especially on the immersion system) is a marvellous test of the correction of the observing objective, but an indifferent one of the image-forming objective used to produce the testing miniature. *These results enable the observer to compare the known with the unknown.* By observing a variety of brilliant images of known objects, as gauze, lace, an ivory thermometer, and sparkles of mercury, all formed in the focus of the objective to be tested with the microscope properly adjusted, so that the axes of the two objectives may be coincident, and their corrections suitably manipulated, it is practicable to compare known delusions with suspected phenomena.

* From εἴδωλον, a false spectral image.

It was then observed (by means of such appliances) that the aberration developed by high-power eye-pieces and a lengthened tube, followed a peculiar law.

Passage of Pigment Cells through the Capillaries.—Some very curious observations on this subject have recently been made by Signor Saviotti, of Turin, and have been recorded in the 'Lancet' of May 28th. Dr. Saviotti being engaged in studying the inflammatory process in the web of the foot of the frog, first obtained a circumscribed spot of inflammation by means of a drop of collodion, and after a few days found the pigment cells of the irritated spot accumulated around the vessels in a contracted condition, and in the course of a short time that they had entirely disappeared. He immediately applied himself to the question of explaining the mode of their disappearance. In other frogs he excited inflammation by dropping on the web a small quantity of a 2 per cent. solution of sulphuric acid. Again, after a few days, he saw that the pigment cells had accumulated around the blood-vessels, and that, though they still preserved their contractility, their processes were less branched and numerous than natural. On further examination, he now observed that these processes began to penetrate the walls of the adjacent capillaries and small veins, causing an obstruction to the onward movement of the red corpuscles on their proximal side, while a clear space was observable on their distal side, occupied only by serum. And now one of two things occurred: either the process of the cell broke off, and was swept away by the blood current, or the whole cell gradually squeezed itself through the capillary wall (the part within the vessel becoming greatly attenuated and elongated) until it also was carried away. In the former case, the cell, shorn of part of its substance, still remained outside the vessel; in the latter, it of course disappeared entirely. As regards the time occupied in these phenomena, Dr. Saviotti finds that the cell processes penetrate the vessels in a period varying from three to six hours, and that it takes about the same length of time for the whole cell to follow and to be washed away from the internal surface, to which it long remains adherent.

Bivalved Entomostraca from the Coal-measures of South Wales.—Some very important papers have lately been contributed by Professor Rupert Jones to the pages of the 'Geological Magazine.' The papers, dealing as they do with the identification and description of several species, are too long for abstract, but those interested in the subject will do well to refer to them. In the 'Geological Magazine' for May, one of these papers is accompanied by a handsome plate.

The Fibres of the Pregnant Uterus.—A letter, dated June 4th, is contributed to the 'Lancet' by Mr. H. Bengafield, in which the writer puts a question that may possibly be answered by some of our readers. It is as follows:—Kölliker, in his 'Human Histology,' describes the development of the fibres in the pregnant uterus thus:—"Instead of a length of 0·002—0·003‴, and width of 0·002‴, they attain in the second half of the sixth month a length of 0·1—0·25‴, and a width of 0·004—0·006‴. *Consequently their length is increased from seven to*

eleven times, and their width from twice to five times." The latter part of this quotation I find is copied *verbatim* into nearly all the standard works on anatomy and physiology extant. But instead of an increase in length of *seven* to *eleven* times, the above figures indicate an increase of *fifty* to *eighty-three* times, and the breadth from two to three times. Now, which of these widely-differing statements is the correct one is not easy for the student to decide. Therefore if some reader would set him right he would be obliged.

Zoological and Physiological Import of the Size of the Red Corpuscles of the Blood.—In a memoir on this subject by Professor Gulliver in the part just published of the 'Proceedings of the Zoological Society,' some noteworthy observations occur. His discoveries show that the largest red corpuscles of the blood yet known in several genera of one order of Mammalia are found in the order Edentata, and the smallest in the order Ruminantia. He gives figures of them in *Orycteropus* (the Aardvark of South Africa), in the Napu Musk Deer (*Tragulus*), and in the true Musk Deer (*Moschus moschiferus*). Thus the orders in question can be at once distinguished simply by the size of the red blood-corpuscles. And, still further, several families of this or that order may be thus diagnosed; of which he gives a remarkable and curious proof in the blood of *Moschus*. When this was submitted to his examination by Professor Flower, Mr. Gulliver at once pronounced that, though the corpuscles had the ruminant character, they could not belong to an animal of the same genus or family as the *old Moschus* (now *Tragulus*), and his figures show the differences in point. He confirms the rule, long since discovered by him, that there is a relation, *cœteris paribus*, between the size of the red corpuscles and that of the species of animal in a single family of Mammals; though there is no such relation, as Hewson had shown, in species of different orders,—the Mouse and Horse, *e.g.* As to the comparative smallness of the red corpuscles in the smaller species of a family of Mammals or the class of Birds, he concludes that it is a provision for an increase of the sum of the surface of the blood-disks, as carriers of oxygen in the service of respiration and the production of animal heat, since the aggregate surface of a given bulk of them will be multiplied by their minuteness, and thus the comparatively greater loss of heat by the smaller animals will be compensated, as more fully explained in the report of his Lecture IX., in the 'Medical Times and Gazette,' January 17, 1863, and in another of his lectures abstracted in 'Scientific Opinion,' December 8, 1869. The doctrine that the size of the red corpuscles has relation to the food of the animal—that the corpuscles are largest in the omnivorous species—he utterly rejects, because, among other reasons, the corpuscles are not larger in the omnivorous Pig than in the phytivorous Rhinoceros, Tapir, and Ass, and are as large in the carnivorous Ursine Dasyure as in the omnivorous Perameles. The whole memoir abounds in the results of Professor Gulliver's researches on the sizes of the red corpuscles of the blood, and affords remarkable proof of the value of microscopical observations in systematic zoology. The comparative descriptions and figures in this respect are both interesting and new as regards *Tragulus*, *Moschus*, and

Orycteropus; and when Professor Flower has completed his long-expected report on the anatomy of *Moschus moschiferus*, we shall know the true position in the zoological scale of this rare animal. At all events, Professor Gulliver's observations have triumphantly proved the importance and significance of *size*, both in a zoological and physiological point of view, as determined by very numerous and careful micrometrical measurements of the red corpuscles of the blood throughout the vertebrate sub-kingdom.

The Mucous and Peptic Glands of the Stomach.—It has been customary to recognize a distinction between the ordinary follicular glands of the stomach and those more complex glands which lie in the vicinity of the pyloric orifice, and which are called peptic glands. In a paper, however, read before the Silesian Scientific Society on the 13th of May, Dr. Elstein has formed an opposite conclusion. His observations were made on the cat, pig, dog, and rabbit. The so-called mucous glands of the stomach are chiefly found in the pyloric region, except a zone having a breadth of about one-third or one-half of an inch, which is chiefly occupied with peptic glands. The alveoli of the stomach are lined with columnar epithelium, the cells of which, at first closed, subsequently burst, discharging their contents, and becoming replaced by others which were previously subjacent. The cells lining the mucous glands are short, contain a granular protoplasm, with a nucleus at the lower end. The entire series of physical and chemical characters of these cells show that they belong to the typical cells (*hauptzellen*) of Heidenhain. Four or five hours after a meal the mucous glands appear cloudy and shrunken, whilst they absorb colouring matters, as aniline blue, with remarkable facility. The mucous glands secrete a fluid which possesses digestive powers, converting albumen into peptone.

The Structure of Pontobdella verrucata.—The anatomy of this species has been very fully investigated lately by M. Leon Vaillant, who has given a comprehensive account of it in the 'Annales des Sciences Naturelles' (Zoological part, p. 1, 1870). Of this paper the following excellent abstract has appeared in 'Nature.' The species is so called on account of the proper zoonites or segments of the animal supporting four tubercles, though the *cutaneous* segments or zoonites only bear two. The total number of cutaneous zoonites is sixty-seven. The anterior orifice of the digestive system is placed at the centre of the anterior sucker. The posterior orifice opens dorsally just in front of the posterior sucker. The skin presents a dermis and an epidermis, the latter being composed of a delicate cuticle and of a layer of epithelial cells, corresponding to the pigmentary layer of Moquin-Tandon. The dermis is composed of cells concealed by a network of what appear to be anastomosing tubes. Beneath the skin, and almost forming part of it, is a dense layer of smooth muscular tissue, the external fibres of which are circular, the deeper longitudinal. By the agency of these the locomotion of the animal is chiefly effected. Between the muscular layer and the digestive tube an immense number of yellow granules are found, which appear to be of the same nature as the uni-

cellular glands described by Leydig, possessing fine ducts, that can in some instances be followed to the skin, and therefore almost precluding the idea of their being hepatic organs. The nervous system presents twenty-two ganglia, excluding the œsophageal collar; the last one is the largest, and is found in the anal sucker. No eyes have been discovered in them, and their relations to the outer world appear to be restricted to those derived from the sense of touch. The digestive organs present no remarkable deviations from that of the leeches in general. Its divisions are a proboscis, with its sheath; a crop; the gastro-ilial portion, and the rectum. The jaws are reduced to three minute projecting points. The crop extends quite to the posterior part of the body, and presents a series of constrictions. The gastro-ilial portion is a single tube lying above the *cul de sac*, formed posteriorly by the ingluvies, and appears to correspond to the true stomach of other animals. The circulation is effected through a closed system of vessels, and the contents of these vessels are colourless, and destitute of corpuscles. M. Vaillant considers that the blood is represented properly by the fluid contained in the general cavity of the body, which contains definite morphological elements. There are four principal vessels, a dorsal, ventral, and two lateral, and these lie in the muscular layers. The dorsal and ventral vessels communicate freely by large branches; the lateral vessels receive their blood from a delicate plexus of vessels distributed on the intestine, which, however, communicates with the dorso-ventral system; and it is probable that an oscillation of the fluid is constantly occurring from one set of vessels into the other. On the whole, the vascular system is much less complicated here than in the leech. The respiratory function is effected essentially if not exclusively by the skin, and there is no special organ for its performance. In regard to the secretions, reference has already been made to the unicellular glands of the skin; and the only others are some peri-œsophageal glands, which are generally considered to be salivary, and the muciparous follicles, which are ovoid vessels, six in number, on each side, placed in the testicular region, and opening externally with a ciliated orifice. The sexes are united in the same individual. The eggs are deposited either separately or several together enveloped in a common capsule.

Is Eozoon of Animal origin?—We have received a copy of the memoir presented by Messrs. King and Rowney to the Royal Irish Academy, and reprinted for private circulation by the authors. It is a bulky brochure, and is illustrated by four chromo-lithographs which don't say much for Irish lithography. The whole tone of the paper, from beginning to end, is so distinctly personal that to the impartial student of science it is not pleasant reading. So far as we have studied it, however, it seems to us that the authors' case is as hopeless as ever. Nothing that we can find is sufficiently strong in our opinion to overthrow Dr. Carpenter's opinion that Eozoon is of animal origin. It is almost impossible for anyone who has seen the exquisite specimens which Dr. Carpenter long ago exhibited to the Royal Society to avoid the conviction that Eozoon is a fossil protozoan; and we believe that, with the exception of Messrs. King and Rowney, all histologists

admit this view. We should have been glad to reproduce the "conclusion" part of this memoir, but it is so distinctly offensive that it would be out of place in a purely scientific journal, as it also is most certainly damaging to the authors' case.

The Structure of Chitons.—M. W. Marshall has sent us a reprint of a paper of his on this subject which appeared in the 'Archives Néerlandaises' (t. 4). The author, besides dealing with the question of the homologies of the shell and treating of the opinions of Dr. J. E. Gray and M. Middendorff, goes with some minuteness into the question of the anatomy of the genus. The tests he describes as composed of two parts, covered with an epidermis: the *articulament* and the *segment.* The former is composed of four layers; the deepest of these consists of calcareous prisms placed perpendicularly to the axis of the animal, and further it presents the several zones having different degrees of colour; the second layer is harder and thicker and is composed simply of very fine granules, it is porcellanous, translucent, and of a bluish-white colour; the third layer is, like the first, composed of prisms placed at right angles to the axis, viewed in section they have the appearance of being finely striated; the upper layer is of a very peculiar nature. The author says that each *articulus buccalis* is seen to consist of ten peculiar triangular bodies whose points converge to form the point of the articulus. Each of these triangles in its turn appears to be formed of a number of needles, so arranged that they help to give the triangular shape to the body, and are themselves constituted like a feather of a shaft and minute crystalline barbs, and possibly barbules. M. Marshall gives a series of figures depicting these singular structures.

Microscopical Anatomy of the Liver.—Dr. H. D. Schmidt, of New Orleans, published in a late number of the 'New Orleans Journal of Medicine' a very diffuse paper on the structure of the liver, and illustrated his views by plates. The author's views are of importance, and his method of injecting the liver is of interest. We hope, therefore, in an early number of this Journal to reproduce a considerable part of his paper. We may mention, however, that the memoir is of great length, and that the author's conclusions differ from those generally accepted in this country. Curiously enough, he sides neither with Dr. Beale nor Dr. Handfield Jones; but, differing from both, he makes the biliary ducts take their origin in a minute plexus of capillaries. He also describes very minutely the lymphatics of the liver.

A New Form of Calamitean Strobilus.—Professor Williamson, of Owen's College, Manchester, sends us the reprint of his paper on this subject, lately read before the Literary and Philosophical Society of Manchester. We could not, without the aid of the handsome plate accompanying the paper, give a just abstract of the author's description, but the following is a general outline of the specimen which Professor Williamson found in the collection of Mr. Butterworth, of High Crompton:—When the specimen came into the possession of Mr. Butterworth, it consisted of but three oblate joints or segments of what had once been a larger structure. In its general aspect it appears

to have closely resembled, if it was not identical with, one which Mr. Binney figured * and referred to as resembling the *Aphyllostachys jugleriana* of Göeppert. Mr. Binney describes his strobilus as being about half an inch in length, and consisting of eight or nine crown-shaped masses or joints, each of which, calculating the proportions indicated by Mr. Binney's figures, must have been about the $\frac{1}{16}$ of an inch in length, and from $\frac{3}{16}$ to $\frac{1}{4}$ in transverse diameter. Mr. Butterworth's strobilus has considerably exceeded these dimensions. The specimen is somewhat compressed laterally; hence its transverse section presents an oval figure. The length of each joint, or internode, supporting one verticil of sporangia is about $\frac{1}{4}$ inch, its greater diameter being $\frac{7}{16}$, and its lesser one $\frac{5}{16}$ of an inch. Two of the three internodes had been sliced into sections before Professor Williamson saw the specimen; but the third, which fortunately happened to be the lowest one of the three, was preserved intact. Externally, each internode of the strobilus has exhibited a series of strongly-marked, rounded, longitudinal ridges and furrows, the former being apparently about twenty in number, though, owing to the fragment being somewhat injured on one side, he could not count them with exactness. These are invested by numerous closely-lapping, thin, membranous, vertical bracts. Each of these bracts appears to have occupied one of the furrows between the ridges, its margins having overlapped, or been in contact with, those of its nearest neighbours along the line of each ridge; but this point also was not very clear, owing to the exceeding thinness of the bracts and the closeness with which they were in mutual contact. The upper part of the fragment having been cut off in making a transverse section, he could not decide whether these bracts terminated with the upper edge of their own internode, or whether their tips were prolonged over the joint above.

NOTES AND MEMORANDA.

A New Preserving Fluid, which has been found useful for anatomical preparation, has been described by M. Méhu, of the French Necker Hospital, and is worth a trial by some of our readers. It only contains a small proportion of alcohol, and hence does not cause any considerable contraction of soft textures, which with him have chiefly been portions of the mucous membrane of the bladder, prostate, &c. It contains arsenious acid, which preserves them from decomposition, and the development of cryptogamic vegetation is prevented by the addition of a small quantity of crystallized carbolic acid. The formula is—Arsenious acid, 20 parts; crystallized carbolic acid, 10 parts; alcohol, 300 parts; distilled water, 700 parts. The preparation of this fluid has led him to notice the great solubility of arsenious acid in alcohol.

* 'Observations on the Structure of Fossil Plants found in the Carboniferous Strata. By E. W. Binney, Esq., F.R.S., F.G.S. Part. I. Calamites and Calamodendron' (Palæontographical Society, 1868), Plate vi., fig. i.

Micro-Photo-Lithography.—Dr. Duchenne, of Boulogne, the well-known advocate of Faradization in the treatment of diseases, lately presented to the French Academy of Medicine a series of photo-lithographs of nerve-sections. According to 'Nature' he stated that he had obtained excellent results from sections of the great sympathetic nerve, the spinal ganglia, the spinal cord, and of the medulla oblongata when magnified from 8 to 500 times. The plan was suggested some years ago by Dr. Duchenne himself; but it was found that the photographs obtained in the ordinary method were not persistent. He therefore fixed them on stone by a process he terms photo-autography, the details of which, however, he does not communicate. It is satisfactory to find him stating that the results of his experiment and photographs only confirm the substantial accuracy of the beautiful drawings made by Mr. Lockhart Clarke on the central parts of the nervous system, and especially upon the medulla oblongata. In his later experiments Dr. Duchenne has adopted Mr. Clarke's mode of preparation with chromic acid and carmine. He states that certain micrographic details come out with wonderful clearness in the photographs, and that by this means some important additions may be made to our knowledge. Thus he has ascertained that in the white substance of the medulla oblongata there are a large number of very small nerve tubules, $0^{mm} \cdot 0033$ diameter, mingled with others of average and of large diameter, $0^{mm} \cdot 01$ to $0^{mm} \cdot 02$ and $\cdot 03$.

American Object-glasses—Mr. Wales' new $\frac{4}{10}$th inch.—The following correspondence relating to Mr. Wales' $\frac{4}{10}$th lately made, and Mr. E. Bicknell's and Dr. Higgins' remarks thereon will, we doubt not, be of interest to Fellows of the Royal Microscopical Society. It is published in the 'American Naturalist' for June:—A performance of a $\frac{4}{10}$th objective made for me by Mr. William Wales, of this city, is of such a superior character that I have no doubt it will be of interest to many of your readers. With direct or central light in contradistinction to oblique, and with the diatom mounted not dry, but in balsam, the *Pleurosigma angulatum* is beautifully resolved; the three sets of lines being brought into view with great distinctness, and this with the No. 1 or A eye-piece. Amplification 210 diameters. With no equal power of Powell and Lealand's of London, of Hartnack of Paris, of Tolles and Grunow of this country, or of Gundlach of Vienna, various objectives of each and all of which makers I have examined, have either I myself, or other microscopists of my acquaintance, been able to effect this. Another feat which I had recently the honour of exhibiting to several members of the "Bailey Microscopical Club" of this city was a resolution of the podura scale with its light central markings with this same $\frac{4}{10}$th. The resolution of the striæ on human muscular fibre by a 3-inch objective, also made by Mr. William Wales, of this city, again challenges our admiration.—J. J. Higgins, M.D., 23, *Beekman Place*, *New York*.

[We referred this note to Mr. E. Bicknell, who kindly sends the following reply.—Eds. of 'American Naturalist.']

Messrs. Editors of the 'American Naturalist,'—In answer to your question in regard to the above communication, I would say that

while fully concurring with Dr. Higgins in his high estimation of Mr. Wales' objectives, I am of the opinion that he (Dr. Higgins) has either made an error in his measurement of amplification (210 diameters with the No. 1 or A eye-piece), or that the $\frac{4}{10}$th objective is very much *underrated in magnifying power*. All of Mr. Wales' $\frac{4}{10}$th objectives which I have seen have been as near or nearer $\frac{1}{4}$ths than $\frac{4}{10}$ths in magnifying power; and below I give a Table of amplification of such $\frac{4}{10}$th objectives as are at hand; also two $\frac{1}{4}$ths for comparison:

Maker.		Angle of Aperture.	Eye-pieces.		
			1.	2.	3.
		°			
4–10	J. Zentmayer	75	130	210	400
,,	Smith and Beck ..	60	135	220	415
,,	R. B. Tolles	135	125	205	390
,,	W. Wales	110	175	300	535
1–4	R. B. Tolles	120	200	325	615
,,	Smith and Beck ..	75	210	340	650

The measurements were made with a first-class stand and eye-pieces of Zentmayer, the image of a stage micrometer being thrown down by a Spencer's camera lucida, and measured at just ten inches from the eye; cover adjustment for 125th cover glass. It seems to me that there should be some uniform standard adopted by the different makers of objectives, so that the $\frac{1}{4}$th of one maker may not be as high as the $\frac{1}{6}$th of another maker; or a $\frac{4}{10}$th of one be as high as a $\frac{1}{4}$th of another; or, still worse, a 3-inch objective of one maker of *precisely the same power* as a 2-inch of another maker, which was just the case with two objectives which I had about one year since. If the objectives did not differ any more than the first three in the above Table it would be an improvement. The amplification which Dr. Higgins gives to his $\frac{4}{10}$ths is as high as the highest $\frac{1}{4}$th in the above Table.—Edwin Bicknell, *Salem*.

CORRESPONDENCE.

Mr. Stodder's Letter.

To the Editor of the 'Monthly Microscopical Journal.'

Dr. Henry Lawson, Boston, *May* 30, 1870.

Dear Sir,—I have but just seen the Monthly Journal for May, my copies for April and May not having arrived yet.

Please publish the following errata in my letter in the May number:—Page 257, 13th line from bottom, 1868 instead of 1848; page 268, 14th line from top, Bicknell for Micknell; page 259, 5th line from top, 19th band instead of 17th.

Yours most respectfully,

Charles Stodder.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *June* 8, 1870.

Rev. J. B. Reade, M.A., F.R.S., President, in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations to the Society was read, which included Part 2, vol. clix. of the 'Philosophical Transactions' from the President.

The usual vote of thanks was given to the respective donors.

Mr. Hogg stated that he had received a letter from Dr. Maddox, enclosing six photographs of *L. curvicollis*, most of which gave decided indications of beaded structure. As the letter contained points of interest it is given *in extenso* :—

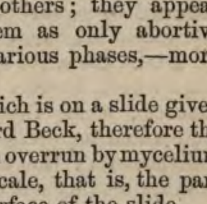
Woolstone, Southampton, *June* 7, 1870.

My dear Sir,—I hasten to send you a few prints by this evening's post (*although very imperfect*), to be in time for your to-morrow's meeting; but to get them ready they have been hurriedly printed on some old paper, as I had not time to obtain others; they appear discoloured. Moreover, you must look on them as only abortive attempts to see the real Podura scale under its various phases,—more especially searching for the beaded structure.

The negatives are all from the same scale, which is on a slide given me by my much-respected friend, the late Richard Beck, therefore the genuine kind; unfortunately the slide is now much overrun by mycelium threads, and one crosses the best part of this scale, that is, the part lying close to the cover, but beneath it on the surface of the slide.

I have used for the photograph marked A, Wales' $\frac{1}{8}$ and achromatic concave with a $\frac{4}{10}$ achromatic condenser, and my usual solar microscopic mirror, large and small condensers with a plate of ground glass interposed at the focus of the collecting lens. For B and C the same, with a double plano-convex condenser $1\frac{1}{4}$ inch in diameter, with a stop over the top lens of the shape Fig. 1, and the small lens of the solar condenser removed. For D the same as B and C, substituting a Nachet's small prism of 30° angle, and replacing the small solar condensing lens. For E and F the $\frac{4}{10}$ achromatic condenser with the large and small solar lenses, and a $\frac{1}{12}$th made by myself—after Mr. Wenham's published formula—with an achromatic concave. The weather was

Fig. 1.



* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

too windy to obtain sharp pictures, causing considerable vibration to the large mirror.

I will spare you a recital of the many trials I have had, and the difficulty to secure anything like the said beaded structure; and remember I have not attempted to obtain what is ordinarily supposed to be the optician's appearance;—just the reverse, everything else except the right ones you'll say,—so be it.

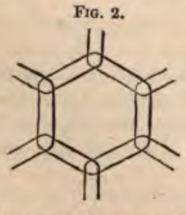
Now the appearances recorded are most fleeting, and required the hand to be on the pinion of the horizontal and vertical adjustment of the mirror to the moment of putting the sensitized plate in its place. Unfortunately I have had sent me some miserable collodion, which would not intensify after the usual methods, so that to procure printing density I had to resort to all sorts of expedients, and on so doing *secured much obscurity.*

I don't think we are yet in order as regards the true structure of this scale, and fancy analogy from other scales should enter into the debate, for to describe the true structure of a transparent object by transmitted light is most difficult: not that I am going to open the question by affirmation; this I leave for wiser heads, with better appliances.

However, to start from a point, and as I shall not be hanged for thinking, I may say that the testing of the scale under very varied illumination, and my never having been satisfied with the appearances obtained by myself photographically, I fancy the following may meet my undecided views.

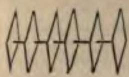
I take the scale to be a truly ribbed structure, finely beaded on the ribs, which deviate or undulate, approach to or widen from one another; when these undulations are focussed so as to give the approximating parts of the upper and under surfaces, you have the undulation of the ribs, converted into notes of admiration and the beading lost, except under most favourable states of illumination and perfection of the objective corrections. The undulations give the waviness, and by slight obliquity in the illumination are thrown from the ribs farther, so that diffraction comes fairly into play, and may produce anything according as the pencils traverse the scale at various angles to the undulating ribs. The prominent round beads shown at some parts of the focus at the head of the notes of admiration, may then be due to the union of the ribs at the points where the widening springs from the same, as is seen so continually in photographing objects with large hemispherical bosses, at the bases of the apparent or made to appear hexagonal areas, when they touch each other, which is shown beautifully in a photograph of *Coscinodiscus* taken with Mr. Wenham's $\frac{1}{12}$ long since. See Fig. 2.

FIG. 2.

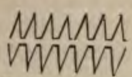


Now whether this ribbing should be considered as a true rib set at right angles to the plane of the intervening structure, and that intervening [illegible]ture a single membrane, or whether the membrane be dou[illegible]ib seated on its surfaces, one on either side, or whether [illegible]*actly opposite* one another,

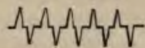
I cannot say, but somewhat incline to the former, so that a transverse section of the scale across its length would, in *exaggeration*, be somewhat after these figures :—


Single membrane.

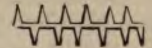
or


Double membrane.

or

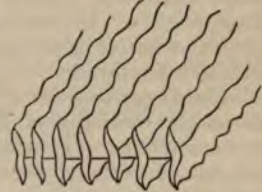

Single membrane. Ribs not opposite.

or


Double membrane. Ribs not opposite.

And if you suppose these ribs carried in the direction of the length *rather obliquely*, and their edges not truly parallel, but waved at short intervals, as in the Figure 3, you may have almost every variety of appearance you desire under various kinds of illumination and focus :—

Fig. 3.



In attempting to photograph this object I had considerable annoyance from the almost immediate loss of the forms obtained on the card screen, from which I supposed some change in the position of the little scale from the extreme heat at the focus of the condenser. Sometimes I fancied the scale curved from its attachment to the cover at both ends, sometimes from one, but this may have been fancy, though I can assure you both eyes and arms ached ere the day's work was over, the former close to the card with hand magnifier, the latter stretched out to the adjusting pinions of the mirror.

So much, then, for these attempts, without an "aplanatic searcher," of which I have seen no account, but which I expect will set all these hallucinations at rest, when I can obtain a peep at the object *à la* Pigott, who, by the way, deserves our thanks for rousing us from supposed security.

Believe me very faithfully yours,

R. L. Maddox.

Jabez Hogg, Esq., Hon. Sec. R.M.S.,
1, *Bedford Square, London.*

A vote of thanks was unanimously given to Dr. Maddox for the photographs.

The President then called upon Mr. James Bell to read his paper "On Fermentation and Parasitic Fungi."

Mr. Slack said it was impossible to do justice at that moment to the very elaborate paper which had been read by Mr. Bell, as it comprehended a great number of details which could not be dealt with until it had been carefully perused. Several of the facts which Mr. Bell had adduced tended to confirm some remarks which he (Mr. Slack) had made long ago on this very subject. The Fellows would probably remember his allusions to the amazing multitude of *bacterium* bodies in the vinegar plant ; and also the evidence which he adduced to show that this plant performed the twofold function of inducing both vinous and acetous fermentations. He noticed that Mr. Bell

had followed French writers in referring *vibrios* to the animal kingdom; but in this he (Mr. Slack) could not agree with him, for he had no doubt that they more properly belonged to the vegetable kingdom. If not, it was clear that the vinegar plant must be regarded as a true *Zoophyte*, with plant and animal organization mixed together. He wished to make an observation on the term "Catalytic action," which Mr. Bell had used. He could not help thinking that the word Catalysis had been employed by chemists to cover ignorance, and no intelligible meaning could be assigned to it. Alluding to Mr. Bell's remarks on *Penicillium glaucum*, in which he contradicted the opinion that it was one of the forms of the yeast plant, he (Mr. S.) did not think Mr. Bell's observations would invalidate the ordinary belief of Fungologists. He nevertheless desired to bear his testimony to the general value of Mr. Bell's paper, and the great amount of research which it displayed.

Mr. Hogg said he felt convinced that if we desired to obtain a thorough knowledge of these fungoid processes it could only be done by combining the knowledge of the chemist with that of the microscopist. But as opposed to what had fallen from Mr. Slack, he had very recently attended a lecture delivered by a learned professor of chemistry, in which the lecturer undertook to demonstrate that the yeast-cell was an animal cell; must no longer be regarded as belonging to the vegetable kingdom. He begged to thank Mr. Bell for the opportunity afforded him of witnessing the various experiments which had just been related, and which had removed from his mind some erroneous views hitherto entertained on this subject. He (Mr. H.) questioned the use of the term *spontaneous generation*. The term used should express the *mysteriousness* which seemed to surround some of these processes of fermentation, rather than that of conveying any idea of spontaneity. Many of the mysterious phenomena in experiments like those recounted by Mr. Bell, had resulted from carelessness in conducting the experiment; for instance, that of not boiling a solution sufficiently long to destroy the germs contained in it. The destructive process employed by Mr. Bell enabled him to say that nothing like life remained behind to set up further change or ferment.

Mr. Brooke said he understood the term "spontaneous generation" to mean the development of organic germs of some kind, independently of and without the pre-existence of any germ developed from organic matter. He believed Mr. Bell had used the term in a different sense to this, since he had alluded to the pre-existence of germs which produced the organisms.

The President said he understood Mr. Bell to say that he had never found one fungus pass into another, Each fungus retained its own peculiar character throughout the whole of its existence.

Mr. Bell, in rising to acknowledge a vote of thanks, remarked that he could not exactly agree with the remarks made by Mr. Slack with reference to *vibrios*. He was of opinion that they seemed to have a determinate object in view, and also appeared to have some powers of locomotion. They could not therefore be classed among vegetables,

for in vegetable organisms such characteristics as he had particularized were not present. With regard to the use of the word "Catalytic," he had followed Liebig. He believed that many of the results obtained in his experiments favoured both the theories of Pasteur and Liebig, while in others the theories of Pasteur could not be maintained.

The President announced the election of Mr. Hankey, of Chicago, as an Honorary Fellow of the Society, and read the following extract from Mr. Hankey's letter of the 21st March last, with reference to his (the President's) allusion to the State Microscopical Society of Illinois:—"I feel a deep personal interest in the greeting which, through you, London extends to Illinois. Regarded simply as an evidence of courtesy and good-will it is most cheering. But in a broader sense, while serving as a reminder of the universal brotherhood of science, it yet possesses the far higher and nobler mission of promoting and cultivating (quietly and unostentatiously it is true, but none the less surely) the good feeling, harmony, and Christian intercourse which should exist between two nations so closely and indissolubly connected by the ties of kindred and of a common language—a mission more effective for the desired results than all the arts of diplomacy. If compatible with your rules I shall take pleasure in being admitted to membership." (Applause.)

Mr. Stephenson then gave a description of his new form of binocular microscope, stating that while it possessed the two important properties secured by Mr. Holmes in his Binocular—*viz.* equal inclination of the two bodies to the object, and the same amount of illumination in each field of view—the image of the object was erect and capable of being viewed by several persons in succession by merely turning the binocular body to each party sitting round a table.

Mr. Brooke said that from his observation of a fly's tongue exhibited under Mr. Stephenson's microscope, it appeared that the definition was not at all interfered with by the peculiar construction of the instrument. The two fields presented to the two eyes were equally illuminated by the two pencils of light diverging equally from the object, instead of being thrown off obliquely, as in Mr. Wenham's arrangement. The binocular effect produced also is more perfect than in the ordinary instrument. The two horny processes by which the fly's tongue is articulated, and the series of tubes of which it consists were very well defined.

Mr. Slack said he had been struck with the perfection of the definition given by Mr. Stephenson's microscope, and the equality of the illumination in each tube when low powers were employed. With a power of $\frac{1}{3}$rd only part of the field was illuminated, but that portion afforded most admirable definition, and no noticeable errors were occasioned by the light impinging upon the edges of the prisms. It was certainly a very valuable invention.

Mr. Lee said he wished to correct the impression that Mr. Stephenson's microscope could be satisfactorily employed only with low powers. He had made examinations with Gundlach's $\frac{1}{16}$th immersion lens (about equal to a $\frac{1}{13}$th of the best English makers), which were quite satisfactory, the definition not being interfered with in any way.

He believed that Mr. Stephenson had brought before the Society the most important improvement in the microscope since the days of Mr. Wenham.

Mr. Browning briefly explained his reasons for believing that the second prism used did not produce imperfect definition.

The President said that he would confirm Mr. Lee's observations concerning the use of high powers in Mr. Stephenson's microscope. The $\frac{1}{16}$th had been used with great advantage, and though both the fields of view were not equally illuminated under this high power, it would at once be seen from the construction of the instrument that only the art of the optician was required to make both tubes equally valuable in using high powers. Those who had had the pleasure of examining the instrument as well as himself would bear him out in the statement that the effect of giving an erect image is of the highest importance in cases where dissection was being conducted. The instrument devised by Mr. Stephenson contained no error of importance, and the slight want of flatness in the field could be readily obviated.

A vote of thanks was then given to Mr. Stephenson for his valuable communication.

Mr. Stephenson, in replying, explained that the spherical effect to which the President had alluded arose from the two planes of the prisms being at right angles to each other. In Mr. Wenham's arrangement the effect produced was cylindrical, but the reflexion of the rays of light at right angles would convert the cylinder into a sphere.

Mr. Beck wished to suggest to the Council the desirableness of inserting in a prominent part of the Journal a statement explaining the benefits to be derived from connection with the Royal Microscopical Society. Many persons, he was quite sure, were desirous of knowing what the real objects of the Society were, and the advantages which it offered to the Fellows. He had been often asked questions on the point, and he thought that if some statement of the kind he had alluded to were issued, the Society would gain very much by the adoption of such a course.

The President thanked Mr. Beck for his suggestion. It would be brought before the Council, and he thought that it would be advantageous to adopt it. The Publisher would be communicated with, and he believed that the value of the suggestion would be recognized.

The Editor of the Journal (Dr. Lawson) expressed his acquiescence in Mr. Beck's proposal, and stated that while the question was one in some measure for the Publisher, the proposed prospectus would have to be drawn up by either the Council or the Secretaries.

A paper which had been received from Dr. Anthony, "On the Appearance of the Frustules in *Pleurosigma angulatum*, and *Pleurosigma quadratum*," also one by Mr. Ray Lankester "On Mr. Sheppard's *Dichroic* Fluid," was taken as read, and a vote of thanks passed to those gentlemen for their communications.

The President announced that the library would be closed during the month of August, and adjourned the meeting to the 12th October next.

The following letter from Dr. Pigott is in reply to some remarks

made by Mr. Wenham upon "definition" in the last number of the Journal:—*

2, LANSDOWNE CRESCENT, W.,
June 8, 1870.

MR. PRESIDENT,—Mr. Slack having referred at the last meeting to a "*long evening*," in which I had the gratification of exhibiting to him some novel experiments on the detection of and measurement of spherical aberration (and chromatic effects), I can only regret that the author of some recent remarks on a paper of mine "On High-power Definition" was not present on that occasion.

The new testing consists of forming on the *immersion* lens focus of the microscope to be tested brilliant images of illuminated disks of light, diminished *two thousand times in miniature*, each disk being accurately formed by apertures $\frac{12}{1000}$ths of an inch in diameter punctured in blackened brass and broached carefully with *Swiss tools*. These apertures are arranged from the 20th, 15th, 10th, and 5th of an inch apart centre to centre. The closest pair *appear* on the stage of the microscope as images, each disk being $\frac{1}{40000}$th of an inch in diameter, and an *aristokratic* image of one disk would be (if the glasses were perfect) exactly $\frac{1}{166666}$th of an inch in diameter, or $\frac{1}{160000}$th of an inch in round numbers.

The closest pair are divided by an interval exactly $4\frac{1}{6}$th (four times nearly) the diameter of a disk.

The finest objectives, such as *I have possessed hitherto*, scarcely divide this artificial star-doublet. The spurious disks of double stars appear swelled out so as to appear almost to coalesce. The lateral aberration in this case is nearly $\frac{1}{40000}$th of an inch.

Mr. Slack noticed that the correcting apparatus WIDELY DIVIDED this new double-star test for microscopic objectives.

Very beautiful rings are visible symmetrically placed just within and without the best focal point, surrounding each of the star-disks forming the "close double."

In the case of the brilliant surface of a recently-broken piece of metal (and in exactly the same manner) every luminous point swells out to a spurious disk, so as to trespass on the images of its neighbours, and the innumerable assemblages of the *spurious disks of residuary aberration* (a subject upon which I propose to write a paper) renders the definition extremely difficult.

Even if the surface of metal, such as steel, iron, copper, brass, silver, and gold, be very highly polished, I can readily develope the appearance of a complex structure *apparently* composed of an agglomeration of spherules separated by dark lines; and the different qualities of polished iron, of which it is known many exist, are thus made evident to the eye of the observer, armed with glasses adequately corrected; especially with an inch or half-inch employed with a power of 800 diameters. A much less power than this is inadequate. But the extreme closeness of the objective to the object under this power *in general* makes the observations *impracticable* with ordinary instruments, as already stated in the REMARKS.

Taking so great an interest in my researches, you will be pleased

to learn that the following thickness of glass has been used for *covers* of objects :—

Objective.	Alone.	With Searcher.
$\frac{1}{4}$th objective.	$\frac{60}{1000}$ths, or ·06 inch.	$\frac{74}{1000}$ths, or ·074 inch.
$\frac{1}{8}$th dry.	$\frac{18}{1000}$ths, or ·018 „	$\frac{24}{1000}$ths, or ·024 „
$\frac{1}{8}$th immersion.	$\frac{20}{1000}$ths, or ·020 „	$\frac{24}{1000}$ths, or ·024 „
$\frac{1}{16}$th dry.	$\frac{7}{1000}$ths, or ·007 „	$\frac{10}{1000}$th, or ·010 „

I have the honour to remain, with great respect,

Yours very faithfully,

G. W. Royston-Pigott.

Rev. J. B. Reade. F.R.S., P.R.M.S., &c., &c., &c.

*Postscript.**—The newly-constructed $\frac{1}{8}$th Powell and Lealand (both dry and immersion) I am happy to add, show diminished spherical and chromatic aberrations. Up to May last considerable residuary errors could be detected in all the best glasses in use. Last year Messrs. Powell and Lealand effected so much improvement in my favourite $\frac{1}{8}$th as to name it a very fine glass. But in comparison with it I have no hesitation in declaring that the effect of their new combinations is simply *superb*. The *difference* between the new glasses and the old sufficiently justify all the criticisms in which I have ventured to confidently assert the *existence of unsuspected residuary aberration in the finest objective of last year, notwithstanding the bold denial they have received.*

Donations to the Library from May 11th to June 8th, 1870 :—

	From
Land and Water. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Scientific Opinion. Part XVIII.	*Editor.*
Quarterly Journal of the Geological Society	*Society.*
Transactions of the Royal Irish Academy. 8 Parts ..	*Academy.*
Medical Progress. By Dr. L. S. Beale	*Author.*
Canadian Journal. Vol. XII. No. 5.	
Moss Flora of Sussex. By C. P. Smith	*Brighton and Sussex Natural History Society.*
Journal of the Linnean Society	*Society.*
Microscopical Anatomy of the Human Liver. By Dr. H. D. Schmidt	*Author.*
Philosophical Transactions. Vol. CLIX. Part 2 ..	*President.*

The following gentlemen were elected Fellows of the Society :—

Thos. Lancaster, Esq.
Chas. J. Morgan, Esq.
Chas. C. Capel, Esq.
and
James Hankey, Esq., of Chicago, an Honorary Fellow.

Walter W. Reeves,
Assist.-Secretary.

* Added June 25th, as the Journal goes to press.—Ed. M. M. J.

Brighton and Sussex Natural History Society.*

April 10th.—Mr. Glaisyer, Vice-President, in the chair.

The receipt of a paper "On Lichens," by Miss Hall, and also of "Notes on a Flint Pebble," by Mr. F. C. S. Roper, F.R.M.S., read before the Eastbourne Natural History Society, from the Secretary, was announced.

It was also announced that the 'Moss Flora' of Sussex was ready for distribution, and that members could have duplicate copies, at a nominal price, by application to the Hon. Sec., Mr. Wonfor, 38, Buckingham Place.

The subject of the fund being raised for the widow and family of the late Professor Sars was introduced by the President. The Society not having power to vote its funds for such a purpose, a subscription was made among the members present, and forwarded to Professor J. Gwyn Jeffrys.

The President, Mr. T. H. Hennah, F.R.M.S., then read a report "On Soundings from the Arctic Seas."

Mr. J. Cook Burrows some years since purchased the geological collection of Sir E. Parry from his widow, and presented the soundings to Mr. J. Peto, by whom they were given to Mr. Hennah for microscopical examination in January last.

The soundings were taken by Sir E. Parry in his expedition of 1818 in Davis's Straits and Lancaster Sound, between lat. 68° N. and 76° 15′ N., and long. 73° W. and 78° 34′ W., in depths between 22 fathoms and 1058 fathoms.

Those from shallow water consisted of fragments of stone and coral much water-worn, having attached zoophytes, a microscopic madrepore, and the tube of an annelid.

From deeper localities the soundings were rich in organic *débris*, much even of the sand being in the form of testæ of Arenaceous Foraminifera of different kinds. Diatomaceæ, particularly large Coscinodisci, were abundant, as were also sponge spiculæ; but shelly Foraminifera and Polycystina were very scarce. Of the Foraminifera, many casts of the insides of the shells were found.

In lumps of hardened mud from Lancaster Sound, lat. 73° N., 674 fathoms, the borings of annelids were found, still containing the skins of their inhabitants, affording conclusive evidence of the existence of life at great depths in the Arctic Seas.

Mr. Hennah regretted that these soundings, which might years since have taught so valuable a lesson, had been allowed to remain without proper examination, until their historical interest, and that attached to recent discoveries, had, almost too late, directed attention to them.

The discoveries of Carpenter, Gwyn Jeffrys, and Wyville Thomson in relation to the physical condition of deep seas—the geological changes going on now as of old—and the addition to our recent fauna of many species before only known in a fossil state, were then alluded to.

* Report supplied by Mr. T. W. Wonfor.

Dr. Carpenter found large areas of very low temperature in the temperate and inter-tropical zones co-existent with high-surface heat. The fauna of these cold areas in low latitudes corresponded precisely with that found in Sir E. Parry's soundings from Arctic Seas.

When the bottom temperature was high the fauna changed its character, the Cretaceous formation going on in warm localities, while in close proximity the greensand is accumulating under privation of heat.

With the increase of our knowledge of the forms of life satisfactory theories respecting its means of sustenance have been offered; the lowest forms—mere atoms of living jelly—absorbing nourishment from the universally-diffused remains of surface life, and in their turn feeding higher organisms.

Carpenter found in the gases separated from deep water a constant increase in the quantity of carbonic acid where life abounded, and a corresponding decrease where it was scanty.

In the ocean, as on the land, food and oxygen—excretion and carbonic acid, are the conditions and consequences of life.

Mr. Hennah concluded by saying that it is impossible to overrate the importance of the discoveries recently made by Carpenter and others, and that he hoped the discussion would turn on them.

In the course of his report he mentioned the common Difflugia of our heath springs and streams as a convenient example of Arenaceous Foraminifera for examination in a living state.

[We much regret that, owing to extreme pressure on our space, several interesting reports of other Societies, though in type, are compelled to stand over.—Ed. M. M. J.]

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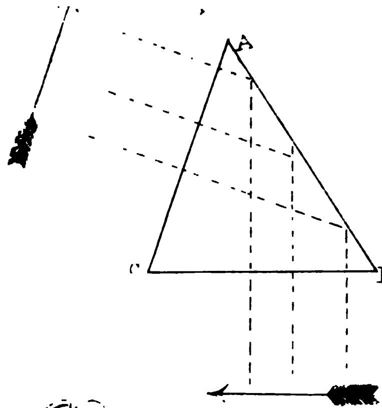


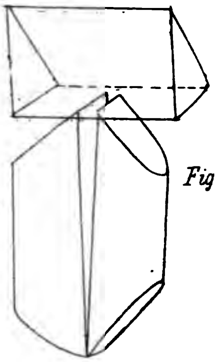
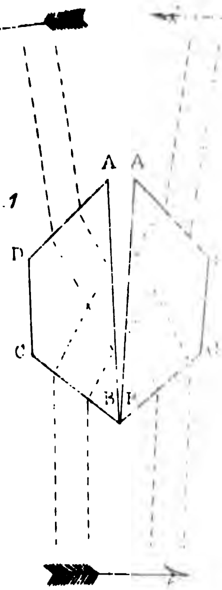


Fig 3.



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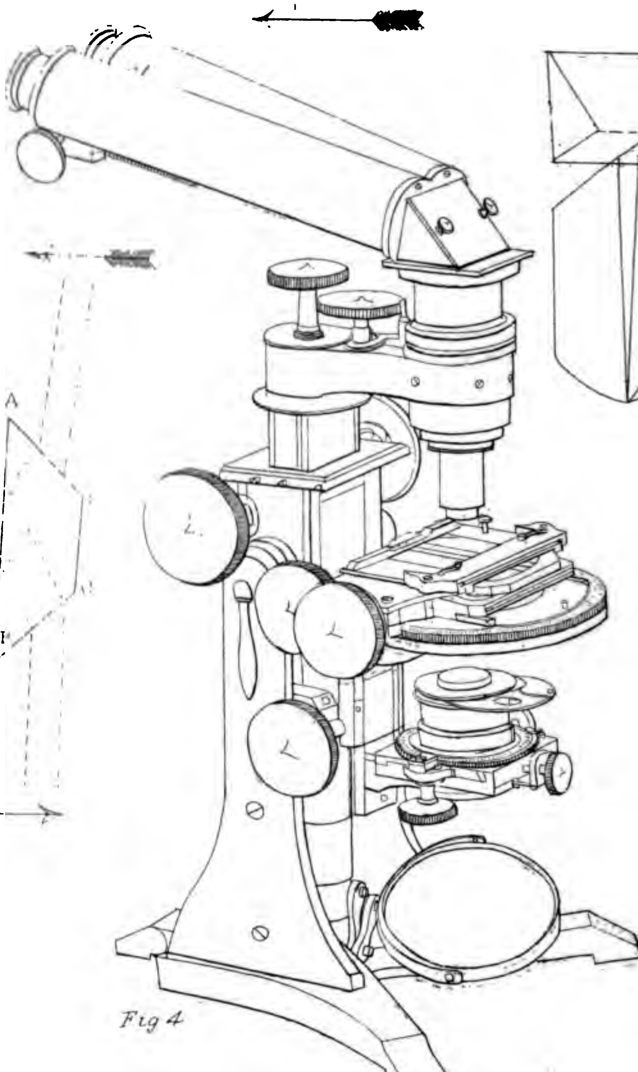

Fig 4

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THE

MONTHLY MICROSCOPICAL JOURNAL.

AUGUST 1, 1870.

I.—*On an Erecting Binocular Microscope.* By J. W. Stephenson, F.R.A.S., F.R.M.S., Actuary to the Equitable Assurance Society.

(*Read before the* Royal Microscopical Society, *June* 8, 1870.)

Plate LVII.

Before I describe the instrument which is on the table before me, it is perhaps desirable that I should briefly point out the purpose for which it was devised, without however dwelling on the great benefits which have unquestionably been derived from binocular vision when low, or moderately low, powers are used.

The inefficient working of the ordinary erector, when monocular instruments alone were known, became more obviously unsatisfactory when Mr. Wenham's beautiful invention was given to the world, for then, not only had we the optical defects and lowering of the magnifying power, which we had previously experienced, but we had in addition, whenever it became necessary to manipulate an object on the stage either in the way of dissection, selection or adjustment, to revert to the old form of instrument, and to make the change necessary in its optical arrangements in consequence, whilst the altered circumstances under which the object was viewed, not unfrequently rendered its recognition difficult, if not impossible.

My aim, then, has been to produce a binocular which, by inversion of the object, both laterally and longitudinally, shall act as an erector, and under which an object can therefore be dealt with without any change in the conditions under which it is originally

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EXPLANATION OF PLATE LVII.

Fig. 1.—The two lower prisms, showing the division and lateral inversion of the cone of light on leaving the objective.

„ 2.—The upper prism, showing the longitudinal inversion of the laterally-corrected image.

„ 3.—The three prisms as arranged in use—the two lower being rounded to fit into the arm which carries the object-glass.

„ 4.—The microscope in its entirety, $\frac{1}{3}$rd of actual size.

observed, whilst at the same time a horizontal stage is preserved, notwithstanding the inclination of the body of the instrument at any angle which by experiment may be found the most convenient in use—the object of the latter arrangement being to enable the observer to dissect, or arrange, objects under fluids, a process which, with an inclined stage, is obviously impossible.

The former object is attained in the following manner.

The light emerging from the objective is received on the shorter sides of two truncated rectangular prisms (Fig. 1), of which the sides enclosing the right angles are equal; the hypothenuse planes of these prisms being placed together, but inclined to each other at an angle of about 4 degrees, form a wedge of glass, having an angle of about 94 degrees, and on this wedge the diverging cone of light falls. The light incident on the first surfaces (B C) of the prisms is thus divided, and being refracted towards the longer sides A B is thence totally reflected and emerges at the upper sides A D; the dispersion caused by refraction at the first, being corrected at the second transmitting surface.

We have thus obtained two fields, each having, by one reflexion, been laterally inverted.

In order to perfect the erection of the image it is now only necessary to induce a second reflexion in a plane at right angles to the first, giving what I have called, for want of a better expression, the longitudinal inversion; this is readily accomplished by placing a third prism (Fig. 2) over those just described, and may be constructed to reflect the light at any angle which, as I said before, may be deemed the most convenient in use, although it is obvious that the nearer this approaches a right angle, the smaller will be the quantity of glass employed, and the less the consequent loss of light. It is sufficiently obvious from the figure that the two laterally corrected pencils of light entering the prism at B C are totally reflected at A B, and emerge longitudinally inverted at A C.

In the instrument before you this angle is 75 degrees (the others being of course $52\frac{1}{2}$ each), which is probably, with this stand, as convenient as any.

By the combination thus complete, the two perfectly-erected fields reach the eyes of the observer at an angle of 15 degrees (being the complement of that previously indicated), which enables him, with his arms resting on the table, to operate with fluids on a still horizontal stage, the distance of which, from the eye-piece, has been lessened rather more than an inch by the bent course which the light has been compelled to travel. At the same time the instrument, being perfectly symmetrical, the illumination of each field is exactly equal, and the focal distance the same.

Having thus imperfectly described the mode by which the two essential conditions have been fulfilled, I will now direct your at-

tention to two other advantages which are, in my opinion, of considerable importance.

The first of these is the much smaller angle at which the two tubes, constituting the body, converge, which arises from the fact that the object being erect, it is no longer necessary that the pencils of light should intersect each other, for the purpose of avoiding pseudoscopic effects, as in the Wenham form; the consequence being that in the erecting binocular this angle is reduced from $12\frac{1}{2}$ to 8 degrees, and the points towards which the eyes converge are situated at the respective distances of $10\frac{1}{2}$ inches and $16\frac{2}{3}$ inches, which distances are proportional to the cotangents of the two angles.

Most of us have probably found that persons unaccustomed to the use of the binocular complain of a straining and aching of the eyes after using the instrument for a few minutes only, and this would probably be found to be the case principally with long-sighted persons, with whom the convergence of the eyes is generally extremely small, and to whom consequently the abnormal condition of vision at a short distance is somewhat painful.

The only remaining peculiarity in the instrument is the rotation of the body on the axis in which the prisms are placed, by which means the stand can, by half a revolution, be placed completely out of the way of the observer, or by its rotation to a greater or less extent the student can place himself relatively to any object on the stage in any position which he may deem the most expedient for his purpose; or, again, when two or three observers wish to examine specimens of interest they can do so successively without rising from their seats.

In conclusion I would only remark that although I have given the angles of the upper and lower prisms as being 90 and 75, and have stated that the hypothenuse planes are placed together, it must by no means be assumed that these angles and positions are the best possible; it may be that as good or even better results might be obtained were the upper prism placed beneath the lower, or were the positions of the lower prisms reversed (causing a still smaller convergence of the tubes), or their angles different from those which I have described, but it appears to me that the excellent definition of the present arrangement leaves little to desire in an erecting binocular microscope.

II.—*Further Remarks on the Oxy-calcium Light, as applied to Photo-micrography.* By Brevet Lieut.-Col. J. J. Woodward, Assistant-Surgeon, U. S. Army.

Since the preparation of my Report of January 4, 1870,* on the use of the Magnesium and Electric lights in Photo-micrography, I have made some experiments with the Oxy-calcium, or Hare's light, as a source of illumination for the same purpose, and have succeeded in obtaining excellent pictures, with powers as high as a thousand diameters. This result appears to me of considerable importance, both because of the comparative cheapness of this light, and because the apparatus for its production is so common as to be practically within the reach of every microscopist. In addition to these advantages the Oxy-calcium light possesses the quality of steadiness to a greater degree than either the Electric or the Magnesium lamp, and requires much less trouble and skill to manage.

For the purposes of my experiments, I made the hydrogen as I consumed it, in a Hare's self-regulating generator, by the action of dilute sulphuric acid on scraps of ordinary sheet zinc. The oxygen was sometimes made in the usual way from chlorate of potassa, sometimes purchased compressed in iron cylinders; in either case it was transferred to a large sheet-iron gasometer for use. The gases were burned under a pressure equal to a column of water 14 inches high. I used for lamp one of the first-class magic-lanterns manufactured by J. W. Queen & Co. (No. 924, Chestnut Street, Philadelphia, Pennsylvania), in which the disc of lime is revolved by clock-work before the burning jet of gas, and a fresh surface constantly presented to the flame. I simply removed from the lantern the lens intended to magnify the image on the slides when the apparatus is in ordinary use, and allowed the cone of light proceeding from the large condenser of the instrument to fall upon the achromatic condenser of the microscope, in the same manner as described and figured for the Magnesium lamp in my Report of January 4th, a reference to which will render any description of the arrangement of the microscope and of the sensitive plate unnecessary in this place.

I employed the ammonio-sulphate cell, as I do in taking Photo-micrographs with other sources of light, but found I could dispense with the ground glass which is necessary in photographing so many objects, if sunlight or the Electric lamp is employed; a large portion of the lime-disc being luminous, the resulting mixed divergent pencil, like that obtained from the Magnesium lamp, does not produce the interference phenomena which result when the tissues and many other objects are illuminated by powerful parallel rays.

* 'Monthly Microscopical Journal,' June.

This circumstance, however, renders the Calcium light inferior to the sun and the Electric lamp, in the resolution of the Nobert's plate and certain lined test-objects.

I did not find the time of exposure differed materially from what I had given in making photographs of the same objects with the Magnesium lamp, and the pictures produced were not inferior to these in quality. This arose from the fact that the greater steadiness of the Calcium light permitted the use of condensers which concentrated the light to a greater degree than I had found advantageous with the Magnesium lamp, and not from equality in the actinic power of the two sources of illumination. I have recently made some experiments with the view of obtaining positive information with regard to the comparative actinic energy of the Electric, Magnesium, and Calcium lamps which I employ. For this purpose, all condensers being removed, the divergent pencil proceeding from each lamp in turn was permitted to fall, for the space of five seconds, on an exposed circular portion of a sensitive plate thirty feet distant. The whole operation was completed in less than a minute, when the plate being developed in the ordinary way three circular spots appeared as the results of the exposures. The spot produced by the Electric light was intensely black, that by the Magnesium of a rich middle-tint, while the circle impressed by the Calcium light was extremely pale. Want of time prevented me from continuing these experiments, and obtaining, as I desired, numerical values for the relative actinic powers of these sources of illumination under definite conditions; this I have, however, regretted the less, as the actual energy of the naked flames is not really the measure of their availability in Photo-micrography; here the question of steadiness, involving, as it does, the possibility of great concentration, plays a most important part, and materially modifies the result.

So far as I know, the Calcium light has never before been successfully employed as the source of illumination for making Photo-micrographs in this country. My friend Dr. R. L. Maddox, however, writes me that it has been experimented with in England by Drs. Abercrombie and Wilson. He thinks they used powers as high as an eighth with pleasing results. This information has directed my attention to the essay of Dr. Wilson in the 'Popular Science Review' for 1867, volume vi., page 54, in which that gentleman gives in detail the process employed by himself and Dr. Abercrombie. He experimented with an oil-lamp and with the Oxy-calcium and Magnesium lights. He says, page 59, of the Oxy-calcium light:—"I can scarcely think it would be used now that the more active light of Magnesium is within the reach of every one." And of the Magnesium:—"The light fails only in steadiness, and if some means could be devised for burning the metal uniformly and at a

fixed point nothing would be left to desire." Dr. Beale* tells us that some of the pictures of these gentlemen were remarkably good, "they possess a peculiar delicacy in the half tones and the shadows, with much roundness of the objects, but the definition, as might be expected, does not quite equal in some of the finest markings prints obtained from sun negatives." A perusal of Dr. Wilson's paper will show that my process differs from his in the use of the following precautions: the interpolation of an ammonio-sulphate cell to exclude the non-actinic rays, the use of lenses specially made for photography for all powers from the $\frac{1}{8}$th down, the use of much larger condensers to concentrate the light, and so to shorten the exposure, and in the case of the Magnesium light, in the use of a clock-work lamp to increase the steadiness of the illumination. Each of these points are in my judgment essential to obtain the best results.

I learn from the same letter of Dr. Maddox that he had himself made experiments with the Magnesium lamp some time before those of Abercrombie and Wilson. He used powers as high as a fifth, and appears to have obtained better results than I supposed anyone had done prior to the publication of my Report. He gives me the following account of his experience:—"The first picture I took with the Magnesium light was done in a very rude way. An inch and a quarter of wire was held in a small spirit flame and advanced by hand as burnt. The objective was Beck's $\frac{2}{3}$rds, the object the sycamore-leaf insect, and about $\frac{3}{8}$ths of an inch of wire remained after use. I sent a print, with a sun print of the same, to the 'British Journal of Photography,' and in the number for July 1, 1864, you will find some remarks by myself and the editors. Now to try and meet any error that might arise from what we may term want of correction, I used the $\frac{2}{3}$rds with the correcting lens, which is excellent for sunlight; the picture was soft, full of half tone, but wanted, as in other pictures I have seen by artificial light, the decision of definition in the outlines." "After this I used the $\frac{1}{5}$th with the little apparatus sketched in Beale's book,† and which, I venture to think, embraces all that is required for its use, provided the condenser has its focus at the burning point, and that the reflector has the same." "With the $\frac{1}{5}$th, fibres of cotton and muscular fibrillæ of boiled shrimp, with several other objects, were taken, but I did not use any higher power, nor indeed pay much attention to the subject, as I gave the preference to the sun-lighted prints and negatives." I give these extracts with great pleasure, as showing the experience in this direction of one of the most distinguished labourers in the field of Photo-micrography, and regret that I was not acquainted with them at the time of publishing my first Report. The method of Dr. Maddox, however, differed from mine in the same essential points as that of Abercrombie and Wilson;

* 'How to Work with the Microscope,' 4th edition, p. 248. † Page 275.

and the peculiar fitness of the Magnesium light for photographing the animal tissues, and those objects generally which require the use of ground glass when sunlight is employed, would appear to have escaped the observation of these accomplished gentlemen, and to have remained unnoted until the publication of my Report.

ARMY MEDICAL MUSEUM,
MICROSCOPICAL SECTION,
June 4th, 1870.

[Appended to his paper, which is really a Report to the Surgeon-General of the U. S. army, Dr. Woodward sends two illustrative photographs. The first, which represents the 6th square of the Möller's type-plate of the Diatomaceæ, taken with Wales's $1\frac{1}{2}$-inch objective, arranged to give thirty-five diameters, will serve for comparison with the photographs of the same object with the same lens, taken by Sunlight, and by the Electric and Magnesium lamps, which were published with his former Report, and are we believe in the possession of the Royal Microscopical Society. The second represents the *Navicula Lyra*, taken with the Powell and Lealand's immersion $\frac{1}{16}$th, arranged to magnify 1000 diameters. Col. Woodward has presented us with very excellent specimens of these, and has sent duplicates to the Royal Microscopical Society, and to Dr. Maddox, of Southampton.—ED. M. M. J.]

III.—*Cursory Remarks on the Podura Scale, Lepidocyrtus curvicollis, and Degeeria domestica, or the speckled variety.*

By R. L. MADDOX, M.D.

A FEW brief remarks are offered to the readers of the Journal on the above scales, as slight explanations of fourteen photographs to be seen, through the liberality of Mr. Baker, optician, at his establishment, 244, High Holborn, London.

The present time being the period of recess, hence the impossibility of early bringing the photographs before the Fellows of the Society in the usual way has induced me to thus afford every facility that they may be seen by those who take an interest in the subject.

The question of structure has yet to be determined by the contending parties; hence at the present time I shall merely copy the remarks appended to each picture, and what I consider to be the view given in the photograph. To attempt to explain each would trespass too largely on the pages of this Journal.

No. 1.—Podura scale (test), view of the ribbed structure generally.

No. 2.—Podura scale (test), view approaching the optician's test appearance.

No. 3.—Podura scale (test), view of markings? beads in the notes of admiration! and general roughness of the scale.

No. 4.—A, Podura scale (test), view of faint markings? beads on the ribs.

No. 5.—A, A, Podura scale (test), similar to No. 4, but rather larger.

No. 6.—B, B, Podura scale (test), somewhat similar view, with rather larger markings shown on the ribs.

No. 7.—I, i, Degeeria domestica (speckled), view of the markings on the ribs? beads and slight waviness of the scale.

No. 8.—J, j, Degeeria domestica (speckled), view of the markings on the ribs? beads, second set showing faintly.

No. 9.—K, Degeeria domestica (speckled), view of the markings on the ribs? beads, rather deeper focus than No. 8.

No. 10.—L, Degeeria domestica (speckled), view of the markings on the ribs? beads, with slight waviness; also in parts the two sets of markings.

No. 11.—M, Degeeria domestica (speckled), view focussed for the best general view of the scale and markings, which Dr. Pigott regards as "the best approximation view of the set."

No. 12.—N, Degeeria domestica (speckled), view focussed to show two sets of markings? beads——

No. 13.—O, Degeeria domestica (speckled), view to show the markings? beads, in the interspaces or the notes of admiration!!

No. 14.—P, Degeeria domestica (speckled), view to show the two sets of markings? beads, as the ribs unite or pass each other obliquely.

The photographs of L. curvicollis are from the same scale on a slide given me by my friend the late R. Beck; those of Degeeria are also from one scale on a slide kindly presented to me by Mr. McIntire. The magnification ranges from 920 to about 1920 in the former, and about 1300 in the latter.

I must trust to the interest in the photographs as compensatory to the above details.

IV.—*Description of a simple Air-Sieve.*

By Metcalfe Johnson, M.R.C.S.E., Lancaster.

In Art. VII., M. M. J., August, 1869, it is stated that "By means of an air-sieve I have collected distilled water trickled over a glass plate into a trough, and found varying quantities of Monas Lens, besides other air-contents."

It is desired in the present remarks to describe this "air-sieve," in the hope that others of your readers may be induced either to employ it and test its accuracy, or to point out the sources of fallacy, and thus bring about a more perfected experiment.

The apparatus I have made use of consists of a small deal box about 12 inches square, having an opening to the front with sloping sides, so as to direct the current of air directly upon the surface of a glass plate, which slopes (at an angle of about 22°) from a vessel containing distilled water, at the upper part of the box, to a trough placed at its base. The water is caused to trickle slowly over the glass plate by means of a cloth siphon from the upper vessel. In its course it comes in contact with the air, which is conveyed in a current through an open window into a room whose temperature is greater than that of the outer air, thus causing a traversing of the air of "out-o'-doors" to the inner chamber. The box contains a smaller opening to the chamber, through which the current can be detected. The water which trickles over the plate is then collected in the trough at the bottom, which, when examined microscopically, shows the presence of Monas Lens upon its surface, and when tested by solutions of potassic permanganate, by means of a volumetric analysis burette, shows varying proportions of organic matter present.

One noticeable fact is that the presence of monads is always, in these experiments, best discovered by examining the surface of the liquid. By the use of water, as in the apparatus described, the particles floating in the air are seized in a way which produces no injury, and the surface brought into contact with the air is larger than in Prof. Angus Smith and Dancer's experiments.

V.—*The Microscopic Structure of the Human Liver.*

By Dr. H. D. Schmidt, of New Orleans.*

Plate LVIII.

The human liver, like that of all vertebrated animals, is composed of three principal parts. The first comprising the blood-vessels, ducts and lymphatics, which convey the fluids either to or from the organ; the second, the special instruments of secretion, *viz.* the cells with free nuclei and granules; the third, the nerves which stimulate and regulate the functions of the organ.

The blood-vessels conveying the blood, both venous and arterial, to the liver, are the portal vein and hepatic artery, which enter it at its inferior surface. Those conveying the blood from it are the hepatic veins, which, taking their exit posteriorly, join the ascending vena cava. The hepatic duct—carrying off the secretion of the

* A reprint of this valuable paper which was originally published in the 'New Orleans Journal of Medicine' (October, 1869), reached us quite lately. As the views Dr. Schmidt enunciates differ materially from the opinions of Drs. Beale and Handfield Jones, who themselves represent opposite schools, we have thought them worth reproducing, at least in part, in these pages.—Ed. M. M. J.

organ,—the nerves and lymphatics always accompany the portal vein and hepatic artery.

The vessels and the duct, after having entered the liver, divide and subdivide until their ultimate branches have become small enough to pass into capillary networks.

The space between the branches of the vessels, &c., is occupied by the parenchyma of the liver, which consists of two capillary networks — perfectly independent of each other — the meshes of which are filled up by the hepatic cells, free nuclei, and granules. One of these networks originates in the ultimate branches of the portal vein and hepatic artery, and terminates in the smallest

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EXPLANATION OF PLATE LVIII.

FIG. 1 represents the termination of the finest branches of the portal vein and hepatic artery in their capillary network, and the commencement of the finest branches of the hepatic duct in the network of "biliary tubules." On the left side the capillary network of the blood-vessels alone is represented; on the right, its relationship to the network of biliary tubules, from which the finest branches of the hepatic duct arise, can be seen. *a*, intra-lobular branch of the portal vein; *b*, intra-lobular branch of the hepatic artery,—both these vessels are seen to terminate in the same capillary network. *c*, intra-lobular branch of the hepatic duct,—its finest ramuscules are seen to arise in the network of biliary tubules. The latter, in order to distinguish them from the capillaries which carry the blood, have been shaded very dark. *d*, transversely oblique section of an intra-lobular hepatic vein, showing the manner in which it receives its capillaries; *e*, capillaries for the blood; *f*, biliary tubules, or capillaries for the bile. The above drawing is a composition taken from four different specimens of sections of human liver. Magnified 90 diam.

„ 2.—Plexus of lymphatics in the capsule on the surface of the human liver: *a*, large lymphatic vessels, showing their characteristic sinuses, dilatations, and valves; *b*, portions of the network of biliary tubules, in which the finest lymphatic vessels are seen to arise; they are imbedded in a small portion of parenchyma which was left adhering to the capsule. Magnified 30 diam.

„ 3.—Small portion from the capsule of the "portal vessels," showing the anastomoses of the small branches of the portal vein and hepatic artery; also the "hepatic glands" and the plexus formed by their ducts. *a*, branch of hepatic artery; *b*, branch of portal vein; *c*, plexus of hepatic glands; *d*, capillary network of the blood-vessels surrounding a portion of the glands. Magnified 30 diam.

„ 4.—Diagramatic drawing, illustrating the relationship of the capillary network of blood-vessels, and that of the biliary tubules to the hepatic cells. *a*, terminal branch of portal vein; *b*, finest branches of the hepatic duct, arising in the network of biliary tubules; *c*, hepatic cells. The latter are seen to fill up the meshes of the network.

„ 5.—Lymphatics of the capsule on the surface of the liver of the sheep. There is a small portion of the parenchyma left in the preparation, holding a portion of the network of biliary tubules from which the lymphatics can be seen to arise. Magnified 30 diam.

„ 6.—*a*, a hepatic cell, partially torn with the needles of the "microscopic dissector," in order to examine its contents; the latter are seen to be drawn out in the form of filaments; *b* represents the same cell completely torn. Magnified 180 diam.

„ 7.—A hepatic cell torn by a needle with a broken point. Magnified 180 diam.

Fig. 2.

Fig. 1.

Fig. 3.

Fig. 4.

Fig. 5.

Fig. 7.

Fig. 6.

Tuffen West sc. W. West i

Structure of human Liver.

branches of the hepatic veins; the other commences independently within the lobule and terminates in the finest branches of the hepatic duct and those of the lymphatic vessels.

In examining a piece of human liver with the naked eye, it appears to be divided into lobules. This appearance is caused by a mere dovetailing of the inter-lobular vessels enveloped in their sheath, with each other. In the hog, and a few other animals, however, the division into lobules is complete; each being separated from the other by a fibrous capsule, derived from the sheath of the blood-vessels. Thus, each lobule in these animals might be considered a liver in miniature.

Like other abdominal viscera, the liver is everywhere (with the exception of its posterior surface) covered by the peritoneum. Prolonged duplicatures of this membrane form four of the ligaments by which the organ is suspended; the fifth, consisting of the remains of the umbilical vein of the fœtus, is contained within the two layers of one of the other.

Besides the peritoneal covering, the liver has a special capsule of areolar tissue, which occupies an important part in its anatomy. At the inferior surface of the organ, this capsule is derived from the sheath of the different vessels as they enter the latter; at the posterior surface it is blended with the areolar tissue surrounding the ascending vena cava. Its office is the same as that of all other fasciæ, that is, to support and connect the vessels and parenchyma of the organ; its structure is not very dense. In some places it is more strongly developed than in others; this is especially the case in the neighbourhood of the hepatic ducts and arteries, where it has a great number of vascular branches and glands to support. On the surface of the organ it adheres to the peritoneum, but, with care, both membranes can easily be separated.

From that part of the capsule which envelops the whole surface of the liver, prolongations proceed over the blood-vessels, ducts, &c., as they enter the organ, and, enveloping them as a sheath of support, are extended to their finest branches, where they at last disappear, by blending with the coats of the latter. One of these prolongations, generally known under the name of the "Capsule of Glisson," encloses the portal vein, hepatic artery and hepatic duct with their lymphatic vessels and nerves; another surrounds the hepatic veins with their lymphatics and nerves—its existence was formerly denied. As these processes of the capsule are strictly analogous in their structure and function, I shall, for the sake of simplicity, style them the "*capsules of the hepatic veins*" and "*portal vessels.*" If there be any existing difference between them it consists only in a stronger development of the latter.

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tions are at present unknown. I refer to extensive plexuses, formed by the ducts of an immense number of small glands, which are either simple or racemose. These receive many fine branches of the lymphatics, and communicate freely with the hepatic ducts. Their extent is very great. We find them at the inferior surface of the liver, covering the space between the large hepatic ducts, before they enter the organ; also in the capsule of the portal vessels —as far as the point where the inter-lobular branches are given off; and in that of the hepatic veins. They are further met with around the ascending vena cava, where it passes the substance of the liver; in this situation I have found a very dense plexus of them communicating with the lymphatics. They exist also in the walls of the gall-bladder.

The *portal vein*, after having entered the liver, divides into two large branches which turn laterally. These, besides giving rise to several smaller ones, divide into two or three branches, one of which takes its course toward the posterior surface of the liver, the other toward its anterior margin, and the third toward its upper surface. From the last-mentioned branches smaller ones arise which are distributed in the same radiating manner throughout the organ. The *hepatic* veins proceeding from the ascending vena cava are usually—beside some smaller branches—two large trunks which turn forward while inclining laterally. From the root of these trunks, a smaller branch springs, which runs in a lateral direction, parallel with the posterior margin of the liver. Each of the two large trunks, while running forward, divides into two or more branches which proceed toward the margin of the liver in a latero-anterior direction. From the last division, smaller branches arise which are distributed, like those of the portal vein, in a radiating manner throughout the organ. The course of the branches of both sets of vessels is inclined toward the upper surface. The large trunks and branches of the hepatic and portal veins cross each other, while the smaller ones run almost parallel. The branches of the hepatic veins are remarkable for their straight course and for the sharp, more or less acute or even right angles which they form with their parent trunks. The course of the *hepatic artery* and *hepatic duct* is the same as that of the portal vein. From the smallest branches of the vessels and duct just described, other subdivisions are formed and continued until the ultimate ramuscules, resulting from this process, have become small enough to join their respective capillary networks.

Of the Arrangement of the Vessels, Ducts, &c., within the different parts of the Capsule of the Liver.

1. *Within the Capsule of the Portal Vessels.*—The *hepatic artery*, while dividing into its larger branches, and before entering the sub-

stance of the liver, furnishes small branches to supply its own walls, those of the portal vein, hepatic duct and lymphatics, and the neurilemma of the hepatic plexuses of nerves with blood, which is returned by similar branches to the *portal vein*. Besides these, there are others given off, which, in company of small branches of the portal vein and hepatic duct, enter the substance of the liver; their distribution will be described hereafter. The duct here receives numerous branches of the ducts of the plexuses of hepatic glands, which have been mentioned above.

The main branches of the *portal vessels*—as we may term the portal vein and hepatic artery with the accompanying hepatic duct—resulting from the divisions of the parent trunks, enter the substance of the liver, and continue to divide and subdivide, while passing toward the surface of the organ, in a manner as has already been described. They are always found close to each other, held together and enveloped by that prolongation or process of the common capsule of the organ, called the "capsule of the portal vessels," which, of course, diminishes in proportion to the calibre of the vessels formed by the various subdivisions, until it finely disappears entirely by blending with the coats of the finest branches.

All the branches of the *hepatic artery* resulting from its various divisions and subdivisions—excepting those finer ones that enter the parenchyma, usually termed "lobular"—send off a set of smaller ones, which are destined to supply the coats of the vessels, the nerves, and also that extensive system of hepatic glands with blood. The size of these branches is proportionate to the calibre of the vessel from which they spring, and the manner in which they are distributed is peculiar. Having arisen from their respective vessels, they divide and subdivide in accordance to their size, and then anastomose with each other on the inner surface of the capsule. From these anastomoses, a number of branches arise which are variously distributed (Fig. 3). Some of them proceed to the coats of the vessels and ducts, and also to the plexuses of nerves; others to the plexus of hepatic glands; and others again pass directly to the parenchyma. This division, however, must not be adhered to very rigidly, as we frequently observe one branch supplying different adjacent parts. Besides those already mentioned, there are others which pass to the *capsule* of the *hepatic veins*, in order to supply the coats of the latter. Lastly, there are some branches which, penetrating to the external surface of the capsule, form there small anastomoses from which principally the smaller inter-lobular and lobular branches take their origin. The arterial branches just described, of course, are accompanied by similar ones coming from the *portal vein*, in order to return the blood carried to the various parts by the arteries. The arrangement of these veins, however, differs a little from that of the arteries. After they have arisen

from their parent trunks of the portal vein, unlike the arteries, they remain not at the internal surface of the capsule to subdivide and form anastomoses, but penetrate at once into the tissue of the capsule. Here, from their subdivisions, an extensive venous plexus is formed, from which, ultimately, all those smaller branches arise that accompany the arterial branches above described.

The *hepatic ducts*, resulting from the various subdivisions of the main trunk—always corresponding with those of the portal vein and hepatic artery—give origin to a number of small branches, the largest of which accompany the inter-lobular veins and arteries, while the smaller ones proceed directly to the parenchyma. All branches of the hepatic ducts—with the exception of those which enter the parenchyma, commonly called "lobular," and also the finer "inter-lobular"—are joined by numerous fine communicating branches from that plexus formed by the ducts of the hepatic glands.

The *nerves* within the capsule of the portal vessels are numerous: they form a complicated plexus, principally around the artery. The diameter of the finest branches, I observed, was $\frac{1}{2100}$ of an inch; accompanying an artery of $\frac{1}{800}$ of an inch; they still formed a plexus. The investigation of the termination of the nerves would necessarily involve a great deal of time. Not being able to devote my attention to the subject for the present, I am obliged to postpone this part to a future period.

The *plexus* formed by the *ducts* of the *hepatic glands* in the capsule of the portal vessels is very extensive. It communicates—as already noticed—by numerous branches with the hepatic ducts, and it is also joined by many branches of the plexus of lymphatic vessels.

The *lymphatic vessels* within this capsule form a plexus which communicates by small branches with the small hepatic ducts and the plexus of the glands; the rest joining each other, enlarge as they proceed until they take their exit from the liver.

2. *Within the Capsule of the Hepatic Veins.*—The branches of the hepatic artery which proceed to the capsule of the hepatic veins divide and anastomose freely with each other; their diameter is about from $\frac{1}{100}$ to $\frac{1}{150}$ of an inch. The anastomoses thus formed give origin to smaller branches of about $\frac{1}{225}$ of an inch diameter, which, by anastomosing with the former, render the meshes of the network considerably smaller and more complicated. Finally, still smaller branches arise from this network, a number of which are seen to terminate in the capillaries of the adjacent parenchyma, while the rest proceed to the fibrous coat of the hepatic veins within which they form a plexus with large meshes.

The portal vein sends a few short branches from which smaller ones arise, which, in their distribution, correspond to those of the artery.

A few small branches of the hepatic duct are occasionally met with, which, however, soon terminate in the "biliary tubules" of the parenchyma.

There are also plexuses of the ducts of the hepatic glands found in the capsule of the hepatic veins; the glands belonging to them are, for the greater part, simple.

The lymphatics of this region are very numerous, and form—as in the other parts of the capsule of the organ—very extensive plexuses.

Of the nerves I can make no definite statement, although I do not entertain any doubt of their presence.

3. *Within the Capsule surrounding the Surface of the Liver.*—Within the substance of the liver the hepatic artery sends off branches of considerable size, which penetrate to the surface. Here they divide into smaller branches—about $\frac{1}{125}$ of an inch in diameter, which, anastomosing with each other, form an extensive network of large meshes. From this network smaller branches arise to form a set of anastomoses of smaller meshes, which, in their turn, give origin to still smaller branches, whose subdivisions, finally, terminate in the capillaries of the parenchyma.

Branches of the portal vein also penetrate to the surface of the liver, where they—without forming any anastomoses—soon subdivide, to terminate, like those of the artery, in the capillary network of the parenchyma.

The termination of the branches of the hepatic artery and portal vein on the surface of the liver in the capillary network of the parenchyma, can readily be seen when well injected. Those of the artery have been described by some anatomists, as terminating in a *capillary plexus* of *large* meshes from which the branches of the vein arise. Upon a superficial examination, it is apparently true that the termination of the artery is a broken network of large capillaries; by minute examination, however, it will be seen that this is no complete network, but only the transition of the ultimate branches of the artery into the capillaries of the parenchyma. Even if this were not proved to be the case, it would be difficult to believe that one vessel should terminate in another, when the current of the blood in both runs in the same direction.

Upon the closest examination of the best injected specimens, I have not been able to detect the slightest trace of branches, sent from the hepatic artery to the peritoneal covering. As, however, the peritoneum is not properly a component part of the liver, there is no reason for expecting that it should be nourished by the hepatic artery. It is very easily separated from the capsule.

Here and there, in the capsule, we meet with small branches of the hepatic duct, which, however, soon terminate in the "biliary tubules" of the parenchyma. I have seen such branches of con-

siderable size at the posterior surface of the liver, where it is devoid of peritoneal covering.

The lymphatic vessels of the capsule on the surface of the liver are numerous, and form a beautiful and extensive network of large meshes (Fig. 2). The vessels composing these meshes are almost as fine, or in some places even finer, than a capillary blood-vessel. Some times one is seen for a short distance to enlarge in diameter, when, joining together, it will be reduced again to its former size. The larger branches, arising from the plexus, join another, formed by larger vessels; and the latter communicate with still larger meshes; the vessels of which can be distinguished by the naked eye. From the latter, those vessels arise that proceed to the lymphatic glands in the vicinity of the liver. Some lymphatic vessels always accompany the branches of the hepatic artery.

The nerves ramifying in the capsule under consideration, follow the course of the artery; I have not traced them to their termination.

In taking a final view of the distribution of the vessels, &c., within the different parts of the capsule, we find everywhere, branches of the hepatic artery forming anastomoses, from which all other branches, destined to supply the different components of the organ with blood, take their origin. The distribution of the portal vein is similar, except in the "capsule of the portal vessels," where the plexuses, formed by its branches, are most abundant in the vicinity of the "hepatic glands," which are exceedingly numerous in this situation, on account of the close proximity to the hepatic ducts into which they empty. The lymphatics are also equally distributed throughout all parts of the capsule.

It is obvious that the main function of the capsule of the liver with its prolongations, is simply to support and bind together, or hold in their respective places, the various vessels, ducts, lymphatics and nerves of the organ; it is, therefore, analogous to the function of the various fasciæ and sheaths of blood-vessels and nerves in general.

Termination of the Portal Vein and Hepatic Artery in the Capillaries of the Parenchyma. Commencement of the Veins. Structure of the Vessels.

Some of the branches arising from the anastomoses, formed by the portal vein in the capsule of the "portal vessels," pass into the parenchyma and give origin to those which finally enter the so-called "lobules"—*imaginary subdivisions*—of the human liver. These inter-lobular and lobular veins are accompanied by one, or sometimes two, similar branches derived from the anastomoses of the hepatic artery. The mean diameter of the former is $\frac{1}{570}$ of an inch, that of the latter, $\frac{1}{1200}$ of an inch; but frequently the lobular branches of the artery are almost as large as those of the vein.

After having entered the lobules, both the branches of the vein and artery send off their ultimate ramuscules from which the capillaries of the blood arise. Thus the portal vein and hepatic artery finally join each other in a common capillary network (Fig. 1). The termination of this network takes place in the smaller branches of the hepatic veins, which were named by "Kiernan," "the intra-lobular hepatic veins." By the union of these, larger branches are formed, which have been termed the "inter-lobular hepatic veins," from the fact that they run between the lobules.

These, in their turn, unite to form still larger branches, until, by repeated junctions, the larger trunks of the hepatic veins are formed. Those venules, however, coming from lobules, adjacent to the various trunks, empty directly into them, without passing first through a series of increasing vessels. Thus, I have observed small hepatic venules, $\frac{1}{350}$ of an inch in diameter, empty into the largest hepatic venous trunks.

In the human liver, it is difficult to define the exact termination of the "inter-lobular" and the commencement of the "intra-lobular" vessels by their diameter or length. The best way of distinction would be to call them "intra-lobular" as long as they continue to receive the capillaries of the parenchyma, and "inter-lobular" when they have ceased to do so.

The average diameter of the inter-lobular hepatic veins is about $\frac{1}{75}$ of an inch. Their course through the parenchyma of the organ is straight. The intra-lobular veins arise almost at right or slightly acute angles from them, and have a diameter of $\frac{1}{250}$ to $\frac{1}{200}$ of an inch. I have, however, seen some branches, $\frac{1}{140}$ of an inch in diameter, still receiving capillary vessels. The intra-lobular hepatic veins mostly terminate in a bifurcation, the branches of which have a diameter from $\frac{1}{1000}$ to $\frac{3}{1000}$ of an inch; but frequently, before terminating, other very short branches are seen arising from them at right angles.

It has already been mentioned that the larger trunks of the hepatic veins run almost at right angles with those of the portal vein and hepatic artery, and that the branches resulting from the first subdivisions of these vessels next assume a course almost parallel to each other, and, at the same time, radiating throughout the organ. The finer branches, however, cross each other again at right angles like the original trunks from which they descended.

Structure.—After the capsule is removed from the portal veins, their walls consist only of two coats. The outer one is thick but of a loose texture, composed of very coarse bundles of fibrous tissue intermixed with the elastic element; the inner one is a thin and dense fibrous membrane, lined by an epithelium, constituting the serous coat of the vessel. The cells of the epithelium are hexagonal, containing a round or oval nucleus with granules; their

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diameter decreases with that of the vessels which they line; in the larger vessels it ranges from $\frac{1}{3000}$ to $\frac{1}{2250}$ of an inch, in the smaller ones it is not more than $\frac{1}{5000}$ of an inch. The finer veins, near their transition into the capillaries, are only lined by nuclei. I have not been able to demonstrate satisfactorily any muscular fibres in the walls of the portal veins. The vasa vasorum, by which they are nourished, are derived from the plexus of small arteries in the capsule, previously described. They penetrate into the external coat of the vessel to terminate in a large-meshed network of large capillaries. The venules arising from these proceed to join that plexus of small portal veins imbedded in the capsule. The limited supply of blood sent to the walls of the portal veins may be attributed to the absence of the muscular element.

The walls of the hepatic arteries, after the removal of the capsule, consist of three distinct coats. The first, or external, is fibrous and more dense than that of the portal veins, especially where it rests upon the middle or muscular coat. This is of considerable thickness, and consists of a layer of longitudinal, and another of circular, smooth, muscular fibres. The internal is the serous coat; it is lined by the same epithelium as the portal veins. The vasa vasorum are very abundant in the external coat of the hepatic arteries. Derived from the small arterial plexus within the capsule, they form a set of anastomoses in the external coat near its outer surface; but, if the outer portion of this coat is raised, a network with smaller meshes is found beneath. These meshes vary in size; neither are the vessels composing them uniform in diameter, some being $\frac{1}{500}$, others $\frac{1}{2000}$ of an inch thick. As the network approaches the muscular layer, its vessels become considerably smaller, their diameter ranging from $\frac{1}{2000}$ to $\frac{1}{3000}$ of an inch. In the muscular coat I have been able to detect only a few loops of vessels and no regular network. The veins proceeding from the vessels just described, join the plexus of small portal veins in the capsule, from which the inter-lobular portal veins arise.

The walls of the hepatic veins consist of the same elements as those of the artery, with the exception that the muscular layers are considerably thinner, and the vasa vasorum not so abundant, the capsule also adheres more firmly to the fibrous coat than in the case of the hepatic artery and portal vein.

As the branches of the vessels become smaller and their walls thinner, the muscular element in those of the hepatic artery and veins is gradually lost. In the same manner, the capsule is disappearing by becoming ultimately blended with the walls of the finer branches.

The investigation of the structure of the finer branches of the blood-vessels is very tedious; the best manner of pursuing it is as follows:—A piece of liver is put under water, and a fine probe—

about $\frac{1}{150}$ of an inch thick—introduced into one of the finer branches of the vessel to be examined. This done, the branch containing it is carefully dissected with the assistance of a loupe—magnifying about 3 diameters—and then removed, together with a small portion of the parenchyma adhering. After the removal of the probe, the fragment—on a glass slide and supplied with plenty of water—is placed on the dissecting stage, and the vessels freed from the surrounding parenchyma by a neat dissection with fine curved needles, under a loupe magnifying about 8 to 10 diameters and by transmitted light. During the dissection, the water should be constantly changed, and the preparation be kept immersed. The vessel being dissected, as far as possible, under this low magnifying power, the preparation is now covered by a piece of thin glass, and ready for study with the higher powers of the compound microscope. To obtain satisfactory results, many such dissections must be made, and the investigator should be provided with plenty of patience and perseverance. The finer branches of the artery can readily be distinguished from those of the portal vein by their smaller diameter and the greater thickness of their walls; also by the presence of small oblong nuclei, which disappear in the finer branches of the portal vein. The walls of the finer branches of the hepatic veins are stronger than those of the portal vein. In such preparations, if carefully made, the transitions of the finest branches of the vessels into the capillaries can easily be studied.

The capillaries of the parenchyma have nothing peculiar in their structure, by which they might differ from those of other organs; their diameter ranges from $\frac{1}{10000}$ to $\frac{3}{10000}$ of an inch; the average is $\frac{1}{4000}$ of an inch. They can conveniently be studied by taking a very small fragment of the parenchyma, placing it on a glass slide, and—being well covered with water—loosening the texture by means of fine needles, but without tearing it too much. This is done by transmitted light on the dissecting stage, and under a loupe, magnifying from 8 to 10 diameters. Thus properly prepared and washed, it is covered by the thin glass and ready for examination with a higher power. Care must be taken that there is sufficient water on the slide to prevent the covering glass from pressing too hard upon the preparation.

Commencement of the finest branches of the Hepatic Ducts and Lymphatics in the Network of "Biliary Tubules."

Independent of the capillary network which forms the connecting link between the finest branches of the portal vein, hepatic artery and hepatic veins, there exists another in the parenchyma of the liver, from which the finest branches of the hepatic ducts and lymphatics arise, to proceed toward the exterior of the organ. To

distinguish the capillary vessels, by which this network is formed, from those which connect the blood-vessels, I have called them in my former paper on this subject "biliary tubules." There is no apparent difference in the character of both sets of capillaries; if there be any, it is in their diameter, which, however, is a difficult matter to determine when the vessels are distended by artificial injection. In thin transparent sections of injected human liver, the diameter of the biliary tubules is sometimes as large as that of the capillaries of the blood, but in the average it appears to be rather less. Finding the capillaries in small prepared fragments of fresh liver varying in diameter, I am rather inclined in comparing them with those of injected specimens to consider the smaller ones as belonging to the biliary network.*

By the union of the smallest branches of the hepatic duct, originating in the network of biliary tubules, the lobular ducts are formed. These, by joining each other, form the inter-lobular ducts; which, in their turn, contribute to the formation of still larger ones. In this manner the junction of ducts is repeated until, finally, the common hepatic duct is formed.

The walls of the hepatic ducts consist of three coats. The external coat in which the small arteries and veins ramify consists of a loose areolar tissue. The middle one is composed of a similar tissue, with the exception of being denser, and containing some smooth muscular fibres; it also lodges some of the hepatic glands and a vascular network, similar to that in the external coat of the blood-vessels. The internal coat is a mucous membrane. The inner side of this membrane, especially in the main hepatic duct and its larger branches, is provided with a great number of small oval or round pouches, or "cul de sacs," as the French would term them. They vary in length from $\frac{1}{30}$ to $\frac{1}{140}$ of an inch, and are from $\frac{1}{50}$ to $\frac{1}{200}$ of an inch wide. Their lower margin is formed by a crescentic fold of mucous membrane, overlapping the sac to some extent. The

* In my article on the "Hepatic Lobule," published in the 'American Journal of Medical Sciences,' Jan., 1859, I have stated the diameter of the "biliary tubules" to be $\frac{1}{18800}$ of an inch. Although this statement was true according to the extent of my knowledge and judgment at that time, my subsequent researches have nevertheless shown me that I had been deceived by appearances. The elements which I took to be "biliary tubules," in my examinations of fragments of fresh uninjected liver, and which I represented in the drawing accordingly (Figs. 10, 11, 12, and 13), I suppose now to have been capillaries, put on the stretch by the needles during the dissection; or, as the specimens were taken from the liver of the hog, they might have been, at least in Fig. 13, fibrils of fibrous tissue. In Figs. 1 and 2, drawings taken from injected specimens, the difference in the diameters of the "biliary tubules" and the capillaries of the blood may be attributed to the latter having been more perfectly filled with the injecting material than the former, though to some extent it is the fault of the engraver, for the difference in the original drawing is not as great as represented in the engraving. In Fig. 3 the "biliary tubules" alone are injected, and, in consequence, the diameter is correct.

largest of these pouches are subdivided into smaller compartments by secondary folds of the membrane in their interior; and it is into these subdivisions that the ducts of the hepatic glands empty. Judging from the position of the crescent-formed, almost valve-like margin of the pouches, it seems that their office is: to arrest the bile—coming from the interior of the organ—until it has been mixed with the secretion of the hepatic glands. In the larger ducts these pouches are numerous and irregularly distributed over the whole mucous membrane; but when the ducts have decreased in diameter to some extent, the pouches become arranged in two rows.

The epithelium, by which the larger ducts are lined, is columnar and $\frac{1}{400}$ of an inch thick; its component cells can easily be observed in their different stages of development; their colour is greenish yellow, similar to that of the hepatic cells. Many of the fully-developed cells of epithelium of the larger ducts have the peculiarity of possessing filamentous appendages of extraordinary length.

The development of these cells seems, at first, chiefly to take place in two opposite directions by filamentous processes, the result of which is a bi-polar cell. The longer process of this cell, which points towards the surface of the epithelium—after having grown to its full length, expands laterally, when the cell has become mature. I have seen a number of fully-grown cells provided with those filamentous appendages above mentioned, whose total length exceeded considerably the whole thickness of the epithelium. In these cases, the appendage must have rested horizontally upon the basement membrane. The fully-developed cells, without filamentous appendages, are $\frac{1}{800}$ of an inch long, and $\frac{3}{10000}$ of an inch wide. When the diameters of the ducts decrease to $\frac{1}{150}$ of an inch, or smaller, the epithelium gradually commences to change from the columnar to the scaly form; and with the further decrease of the former, the flattened epithelial cells also decrease in diameter, until in the finer ones the epithelium consists only of nuclei, closely set together. Its last trace is only a granular layer, lining the short branches of the lobular ducts through which the transition into the network of "biliary tubules" takes place. The outlines of the epithelial cells and nuclei of the smaller hepatic ducts are dark and well defined, which forms a characteristic *by which the latter may easily be distinguished* from the blood-vessels.

The fibrous layer of the mucous membrane of the larger hepatic ducts lodges a network of capillaries with very small meshes, derived from the anastomoses of small blood-vessels in the middle coat.

Origin of the Lymphatics.—The finest branches of the lymphatic vessels in the human liver arise, like those of the hepatic duct, from the network of biliary tubules (Figs. 2, 5); they then join each other to form a plexus of larger vessels which have already been described. In the capsule of the portal vessels, small branches

proceed from this plexus, some of which join small hepatic ducts, and others the plexus formed by the ducts of the hepatic glands. The extensive plexus of lymphatics in the "capsule of the hepatic veins" take their origin from the "biliary tubules" surrounding the intra-lobular hepatic veins; they also communicate freely with the ducts of the hepatic glands. In the capsule on the surface of the liver, the finer lymphatics, after having arisen from the "biliary tubules," anastomose with each other, and then join the network of large meshes already described.

The finest lymphatic vessels present that nodose appearance so peculiar to this system of vessels, of the larger ones. The nodosities, of course, are caused by constrictions in the walls of the vessels; they correspond to the place where the valves, with which these vessels are provided, are situated. These valves, in the finer lymphatics, can easily be seen under the microscope.

The existing communication between the lymphatics and hepatic ducts will be further discussed hereafter.

System of Plexus, formed by the Ducts of the Hepatic Glands.

The different localities in which this singular system of small glands with the plexus, formed by their ducts, is found in the human liver have already been mentioned; it only remains to describe the peculiar character of its component elements, and the relation of the latter to the ducts, lymphatics, and blood-vessels. The small glands, attached to those ducts, which form the plexuses, vary considerably in size and form. Many of them join the plexus of ducts in the form of a simple follicle, while others—perhaps the majority—first combine with each other to form a small racemose gland, before joining the plexus (Fig. 3). The diameter of the follicles ranges from $\frac{1}{1000}$ to $\frac{6}{1000}$ of an inch. Their duct or neck is very fine, usually $\frac{1}{3000}$ of an inch in diameter, but sometimes it is almost as wide as the little gland itself; occasionally the latter has the mere form of a pouch or dilatation. Many of these follicles are sessile, especially the smaller; and often are only so large as to be lined by five or six epithelial cells. Sometimes the duct of the follicle assumes the form of a pedicle before joining the plexus; in such cases the vessel of the plexus which they join is usually very fine and the follicles are then set far apart. The small racemose glands, formed by the union of a number of follicles, also differ among themselves; especially in the size of theirmain duct, through which they are connected with the ducts of the plexus. Usually the size of this corresponds with the number of follicles of which the racemose gland is composed; but in many of them the main duct is comparatively wide, and the follicles are almost sessile upon it, or join it by very short ducts.

The size and form of those vessels, or rather ducts, of which the plexus is composed, vary as much as those of the glands. The diameter of some is as large as $\frac{1}{100}$ of an inch, while in others it is not larger than that of a capillary vessel. They seldom present a uniform diameter for any great length, but are constantly changing. It is not unusual that, while presenting a considerable dilatation in one place, they suddenly diminish in diameter to the size of a capillary vessel. In the walls of the large hepatic duct, before its division, and also in those of the cystic duct and the gall-bladder, the ducts of the "hepatic glands" do not anastomose with each other, but after having proceeded for a greater or lesser distance, and received the smaller ducts of single or of groups of glands, open into the pouches—"cul de sacs" of the mucous membrane, where their mouths can be easily observed. Here the glands are often collected together in bunches, but as often they are seen to join the main duct—which here is mostly of a small diameter—in a single or double row, close up to the orifice in the hepatic duct.

In the capsule, covering the inferior surface of the liver, and between the larger branches of the hepatic duct before they enter the substance of the organ, and also in the "capsule of the portal vessels," the plexuses, formed by the ducts of the hepatic glands, are very extensive; especially in the vicinity of the hepatic duct. I have even found portions of them deeply imbedded in the walls of the portal vein. They are not very numerous in the "capsule of the hepatic veins;" here their glands are mostly arranged in single file.

In front of the vena cava, where it passes the substance of the liver, and below the lowest hepatic veins, I have found another very extensive plexus, formed by the ducts of the "hepatic glands," which anastomoses freely with the plexus of lymphatics. The extent of the whole was 1 inch in length and a $\frac{1}{2}$ inch wide; the greater part of it was enclosed between two layers of areolar tissue, derived from the capsule of the liver. They were also freely supplied with blood-vessels. These plexuses I have carefully traced to the larger lymphatics of that region. The communication between the lymphatics and the ducts of the "hepatic glands" may be studied with the greatest advantage in this situation, and also in the "capsule of the hepatic veins." Very frequently, especially in the plexus near the vena cava, I have observed ducts of considerable size, about $\frac{1}{100}$ of an inch diameter or less, possessing for some distance no glandular appendages, and others, of the same size, with very small sessile or follicular glands upon their walls.

The structure of the "hepatic glands" consists of an extremely thin layer of fibrous tissue, a basement membrane, $\frac{1}{25000}$ of an inch thick—the well-defined dark outlines of which can readily be seen through the former—and an epithelium of hexagonal cells contain-

ing a large round nucleus. These cells differ much in their size; some of them are not much larger than their nucleus. Free nuclei are also seen interspersed among them. The diameter of the largest cells is about $\frac{1}{2000}$ of an inch, and that of their nucleus $\frac{1}{4000}$ of an inch or smaller; the latter contains mostly two or three nucleoli. The epithelium of the larger ducts of the plexus consists also of these cells, whose diameter, however, decreases in proportion to that of the duct which they line. In the finer ducts of this plexus, like in the finer hepatic ducts, the epithelium is *nucleated*, that is, composed only of nuclei.

The "hepatic glands" with their plexus of ducts are surrounded by a network of capillary blood-vessels, derived from small arteries, branches of that arterial plexus within the capsule and terminating in venules going to their respective venous plexus of small vessels, contained in the meshes of the capsule, as previously described (Fig. 3, *d*). Each individual follicle or gland receives a capillary in the form of a loop.

Before closing the remarks on the subject of the "hepatic glands," I will refer once more to their connection with the hepatic ducts. When the latter become so small as not to be surrounded any more by the plexus formed by the ducts of the "hepatic glands," we find that their walls commence to be provided with single glands; that is, the glands, instead of first joining a plexus of large ducts, empty directly into the hepatic ducts.

To study thoroughly the various forms of the "hepatic glands" and their ducts, and the relationship they sustain to the hepatic ducts and lymphatics, specimens, well injected with Prussian blue, are indispensable. In such preparations the epithelial cells can still be observed. For the examination of the structure we have to resort to the uninjected specimens. The locality which I usually prefer for the latter purpose is on the inferior surface of the liver and between the larger branches of the hepatic duct; here the plexus is very extensive. In putting a fresh liver under water and carefully removing the peritoneum from that part, the glands are readily recognized by their yellowish pink colour. They are then dissected out under a magnifying glass of low power and placed on a glass slide. Being well covered with water, they are thoroughly freed from the surrounding fibrous tissue by means of fine-curved needles; this is best accomplished on the dissecting stage under a magnifying power from 8 to 10 diameters, and by transmitted light. Having been thus properly prepared, their components may be studied very satisfactorily.

It frequently happens that while dissecting out these glands, a small hepatic duct is cut, and a small portion of its epithelium escapes in the form of small round yellowish bodies. Precaution must be taken not to mistake these for the glands. The error may be

detected by the difference existing between the epithelial cells, those of the hepatic duct being columnar.

Cells, free Nuclei and Granules, occupying the interspaces of the two Capillary Networks of the Parenchyma of the Liver.

The great irregularity of form, peculiar to the hepatic cells, distinguishes them from most other cells in the animal organism. They represent irregular polyhedrons, the diameter of which ranges from $\frac{3}{10000}$ to $\frac{13}{10000}$ of an inch. When closely examined, while floating in water, a constant change of form, according to the position they assume, can readily be observed. This is owing to the great irregularity of their numerous surfaces, which, undoubtedly have been produced by an unequal mutual pressure of the whole mass. The difference of form in one and the same cell, when viewed from different sides, is very striking; and proves how unsatisfactory and incorrect the results of an examination of the liver cells, in their quiescent state, must be. If, however, the attention of the observer is closely directed to the angles of the various surfaces of a hepatic cell while in motion, they may still be recognized, after it has quietly settled. The walls of the hepatic cells are very delicate, and very difficult to be seen while the latter are floating in water, but may be distinguished during their state of rest. The interior of the cells is occupied by a greenish-yellow and highly-viscid substance. I have convinced myself of the viscid nature of this substance by slowly tearing the cells with the fine needles of the "microscopic dissector." Fig. 6, *a*, represents a hepatic cell, caught by the points of two needles, and partially torn; at *b*, we notice the same cell, entirely severed by the further separation of the points of the needles; its halves are held together by the viscid contents drawn out into a filament. Fig. 7 is another cell, mutilated by a needle with a broken point; its contents are also drawn out in the form of filaments. When cells are torn in this manner, their walls seem not to possess a great amount of elasticity, but break rather abruptly.

Imbedded in the viscid material, contained within the hepatic cell, we find a nucleus and granules. Frequently there are two nuclei, and in some instances I have even observed three. The interior of the nucleus also contains some granules, one or two of which are, usually, larger than the rest, and may be regarded as the nucleoli. In two instances I have observed a granule within the nucleolus. The form of the nucleus is mostly round, or slightly oval; its mean diameter is $\frac{3}{10000}$ of an inch, that of the nucleolus about $\frac{1}{10000}$.

Besides the nucleolus and granules, the hepatic cells of the human liver frequently contain some oil-globules, whose presence I

have, however, not found so common as is usually believed, except in the case of fatty livers. By the addition of ether these fat-globules disappear, while the other granules are not affected at all. The cells then are observed to shrink, and to collect in masses, together with the free nuclei and granules. The fat, dissolved by the ether, is seen in the form of globules or irregular patches of various sizes, distributed over the field of the microscope. Under these circumstances the cells float no longer, but settle instantly, without regard to the amount of water added; they have also lost their transparency, and their delicate outlines have become coarse and dark. The granules seemed to be rendered more distinct.

The free nuclei found among the hepatic cells differ in size, and have the same character as those within the latter. The same may be said of the free granules.

Sometimes the nucleus within the cell is found in a state of division. This, and the presence of free nuclei, would indicate that the multiplication of the hepatic cells might take place, both by free development and by division.

Evidences adduced of the existence of the Network of "Biliary Tubules," in which the finest branches of the Hepatic Duct and Lymphatics originate.

The views of numerous investigators on the microscopic anatomy of the liver, as already mentioned, have always been conflicting in regard to the commencement of the finest branches of the hepatic duct. It would be difficult to find out the cause of this discrepancy in the results obtained by different authorities. And yet, when considering the satisfactory results obtained by them from investigations of a far more difficult nature on other subjects, I cannot but think that the proper means have not been resorted to, or this subject has perhaps not received the attention it deserved.

I will now proceed to prove the existence of the network by means of injection. If either the hepatic artery, portal vein or hepatic veins of the liver are injected with a coloured material, and upon a careful microscopic examination of fine sections of the substance of the liver we discover a capillary network of the same colour as was injected, the natural inference would be *that this network is continuous with the finest branches of the blood-vessel into which the material was injected.* This has been repeatedly done, and no one doubts its correctness. All anatomists believe in the existence of a capillary network which forms the connecting link between the finest branches of the portal vein and hepatic artery and those of the hepatic veins. But what would be the inference, if the same material were injected into the hepatic duct, and *a capillary network of the same colour were discovered by a subsequent*

microscopic examination? It certainly ought to be the same as in the former case. Another explanation, however, might be offered to explain the phenomenon; as, for instance, the possibility of a rupture of the finer branches of the hepatic duct and of the capillaries of the blood-vessels, which would afford an opportunity to the coloured material to pass from one set of vessels into the other. Although inclined to deny the probability, I admit the possibility of such an occurrence in one or even in a few instances. This explanation of the phenomenon, however unsatisfactory as it already is in itself, loses every appearance of probability when we meet by a subsequent examination with the most regular capillary network, of the same colour as the material which was injected into the hepatic duct, and without exception, in scores of instances; or, in other words, in every experiment performed. With such facts before us, we can no longer doubt the continuity of such capillary network with the finest branches of the hepatic duct, into which the coloured material was injected. Again, if we observe the continuity of the network with the finest branches of the hepatic duct, as represented in Fig. 1, *c* and *f*, the evidences are still stronger, for certainly no anatomist who is familiar with the diameters and characters of the branches of the hepatic duct and those of the blood-vessels would ever confound the former with the latter.

There are still other proofs, strong enough to convince even the most sceptical or rather prejudiced mind; for instance: A liver is taken, and its vessels and ducts carefully injected—in the manner as will be described in one part of this treatise—with different colours; for example, Prussian blue into the hepatic duct, yellow into the hepatic artery, red into the portal vein, and white into the hepatic veins. If now, by examining fine sections of such a specimen of liver we recognize the different individual vessels by their peculiar characters as well as by their respective colours, injected into their parent trunks, and also by the relative position they occupy towards each other, and then directly trace the finest branches of each to a network of capillaries of its own colour, what further proofs can be expected of the correctness of my statements? In such a case the branches of the hepatic duct, which were injected with the Prussian blue, are observed to terminate in a blue network of capillaries, while those of the hepatic artery and portal vein are seen to terminate in another of yellow and red, one or the other of which usually predominates.

Frequently we find either one or the other network more or less injected. The cause of this is very obvious. If, for example, the capillaries of the blood-vessels are filled with the injecting material sooner than those of the hepatic duct, the former, being already distended, press upon the latter—with which they interlace,—and thus prevent the entrance of the material which was injected into the

duct, into them. This, however, is not always the case, for there are many places met with, in which we find both sets of capillaries injected with their respective colours (Fig. 1).

If a thin transparent section, in which both capillary networks are injected, be examined under the microscope, the two sets of capillaries can be readily distinguished as they interlace with each other. In making such examinations, the sections should be illuminated from above and below; and for this purpose, a condensing lens of no less than three inches diameter should be used. After the mirror and condenser are properly adjusted, and the section thus illuminated by reflected and transmitted light, the attention of the observer should be directed to one particular spot of the section where both sets of capillaries are injected. By turning the mirror slowly, either reflected or transmitted light may be made to predominate, and the examination carried on very successfully. Examinations of such sections by oblique light are also of great importance.

In comparing the interspaces of the network in a thin section, in which only one set of capillaries is injected with those of another section in which both sets are injected, they will be found larger in the former than in the latter; this shows that the networks interlace with each other.

In very thin sections in which one set of capillaries is injected with the coloured material, and the other only with the uncoloured solution of Canada balsam, the walls of the uncoloured capillaries can be distinctly observed, as they accompany and cross the former, if examined with an objective of $\frac{1}{2}$ or $\frac{1}{4}$ of an inch focal length.

The meshes of the network of "biliary tubules" are very regular, and the walls of the latter can be distinguished without difficulty. The regularity of the vessels of this network, when injected with Prussian blue, exceeds even that of the capillaries of the blood-vessels. I have seen the biliary tubules so well filled with Prussian blue, that they appeared almost black, but were still retaining the regular form of their meshes; this strongly serves to prove that they possess walls as strong as other capillaries.

Sometimes, in examining thin sections, places are met with in which the biliary network is well injected and very regular, while the interspaces are slightly tinged with the colour—Prussian blue. At first sight this might be taken for an extravasation; but by closer examination, it will be found that this tinge is produced by a transudation of the injecting liquid—coloured by the exceedingly fine Prussian blue—from the biliary tubules into the hepatic cells. I have convinced myself of this fact, by putting small pieces of fresh liver into the blue injecting material, and by examining minute fragments of it after some hours had elapsed; in such cases I always found the blue colour in the interior of many of the cells.

Another mode to study the network of biliary tubules is by

examining small fragments of injected liver under water by strong reflected light. The objective used for this purpose must be of a low power, and of the best quality.

If a fragment is taken in which the network of biliary tubules is injected with blue, and that of the blood-vessels—from the hepatic veins—with white, the capillaries of both can be seen as they cross each other. Close study is required for this examination, and due allowance has to be made for the transparency of the colours. Moreover, the character of the networks and the diameters of the interspaces between their capillaries—in places where only one or the other is injected—should be critically observed, in order to compare them with those in other places where both are injected.

I can highly recommend the dissection and examination of small injected fragments under water; the most satisfactory results are obtained from it.

The conclusions which I have drawn, in regard to the network of "biliary tubules" in the parenchyma of the liver, are not the results of mere accidental observations on a few specimens. On the contrary, I have examined many hundreds, nay, many thousands of thin transparent sections, and also small fragments of injected liver. The great rapidity with which I can produce sections of great thinness and large size by means of my apparatus enabled me to do so. If well-injected portions of the liver are examined in the manner I have indicated, my statements will be found correct in every instance.

In the fresh uninjected specimens, the commencement of the ultimate branches of the hepatic duct in a capillary network can also be demonstrated; this is, however, attended with more difficulties than the demonstration by injection. It is done by the aid of the microscopic dissector as follows:—A portion of fresh liver is put under water, and a very fine probe introduced into a small branch of the hepatic duct; the latter is then carefully traced to its finer ramifications—under a magnifying power of 3 to 4 diameters—by separating it from the surrounding parenchyma. When it cannot be pursued any further without the risk of tearing it, it is separated from the whole with a very small portion of that parenchyma to which it adheres. The fragment is now placed on a glass slide, provided with a little spring, which, by pressing upon the duct, prevents the fragment from being displaced. Well covered with water, it is placed on the dissecting stage, and under a magnifying lens of $\frac{8}{10}$ths diameter, dissected by means of fine curved needles. The object in view is to trace the duct to its finest branches as far as the low magnifying power will allow; and this is accomplished by a mere loosening of the tissues, in order to liberate the cells from the capillary vessels without tearing the latter. The fragment should be washed several times. Being thus far dissected under a

low power, it is put upon the glass plate of the "microscopic dissector," and kept in place by the small lever. Before commencing the dissection with this instrument under the compound microscope, it is advisable to re-examine the preparation under the simple magnifying lens on the dissecting stage, taking care that it is nicely spread upon the glass, so that the ultimate branches of the hepatic duct, or of the blood-vessels, do not overlap each other. The microscopic dissector, with the preparation upon its glass plate, is now placed upon the stage of the compound microscope, and after a small branch of the hepatic duct—with some of the parenchyma still adhering and rendering it favourable for research—has been found under an objective of $\frac{1}{2}$-inch focal length, the dissection with the curved needles of the instrument may be commenced. After the needles have been made to take hold of the preparation at the proper places, an objective of a higher power may be employed. The curved points of the needles, of course, must be short enough not to interfere with the approach of the objective in bringing the object into its focus.

If a dissection, as just described, is made successfully, the finest branches of the hepatic duct will be seen to commence in a capillary network. A fine duct may be seen in company of its corresponding portal vein and hepatic artery, and, likewise, their finest branches, each originating in its respective vessel, and terminating in the capillaries. The ducts can always be distinguished from blood-vessels by their *characteristic epithelium*. By moving the needle which holds the finer extremity of the duct, it will be observed that the meshes of the "biliary tubules," arising from it, move also, while the capillaries, arising from the blood-vessels, remain stationary; this manœuvre, if well executed, removes every doubt which might still exist in regard to the origin of the finest hepatic ducts. A great deal of patience and manipulative skill, however, are required to make such delicate dissections, and the investigator must not become discouraged when he finds that his labours are not always crowned with success. All he can do when failing, is to try it again.

When the biliary tubules have once been satisfactorily demonstrated by the skill and perseverance of the investigator, he will be able to recognize them—without the aid of the microscopic dissector—in preparations carefully made on the dissecting stage.

The commencement of the finest branches of the lymphatics in the network of "biliary tubules" can only be demonstrated by injection. In removing cautiously the parenchyma, in which the "biliary tubules" are well injected with Prussian blue, from the "capsule of the portal vessels," and then dissecting, under water, a thin layer from the outside of the latter, the lymphatics can often be seen arising from very fine vessels, derived from the "biliary

tubules" of the adjacent parenchyma; they may be recognized by their characteristic constrictions and by their faintly blue colour. The latter fact is easily explained when we consider that the greater part of the Prussian blue, with which the injecting matter is coloured, is retained in the hepatic ducts and in the network of "biliary tubules;" when the material reaches the lymphatics the amount of colour is so small as to render them only faintly blue.

In thin sections of injected human liver we frequently meet with longitudinal sections of intra-lobular hepatic veins, in which the greater part of the vessel—and with it the colour with which it was filled—is removed. In such instances the finer lymphatic branches, running parallel with the vein, can often be seen, as they arise from the biliary tubules.

Sometimes, in minute dissections of the smaller branches of the portal vessels and the duct, fine lymphatics are seen, as they come from the parenchyma to accompany the vessels and ducts.

The most satisfactory observations I made to determine the origin of the lymphatics in the network of biliary tubules was in the liver of the sheep. The results of these experiments were so conclusive that they removed all doubt from my mind in regard to this subject. They were as follows:—An uninjured liver of the sheep was taken, and a small canula, with an orifice of about $\frac{1}{40}$ of an inch in diameter, inserted into the hepatic duct. Being placed into the basin of my injecting apparatus, the canula was connected with the latter, and everything else prepared and adjusted in order to inject it in the manner as described in that section of this memoir which treats on the subject of "minute injections," with the description of the apparatus. By exhausting the air from the interior of the basin, it is also exhausted from the interior of the hepatic ducts; at the same time, its pressure is removed from around the organ, and also from the cut ends of the large lymphatic vessels, which emerge from the liver alongside of the portal vessels, and those coming from those lymphatic glands near the vena porta. As soon as the communication between the interior of the hepatic ducts and the interior of the basin was cut off, and that with the injecting liquid—coloured with Prussian blue—established by a slight turn of the intervening stop-cock—the liquid in the glass tube commenced to descend very slowly by its own weight and the pressure of the atmosphere. Soon after, the patches appeared at the surface of the liver, indicating the arrival of the blue injecting material in the "biliary tubules" of those places; but at the same time, it also issued from the open mouths of the large lymphatic vessels. When the vacuum in the basin was destroyed by the re-admittance of the atmosphere, the pressure of the latter upon these open lymphatics arrested the flow of the coloured liquid from their cut ends, and also its descent in the glass cylinder; this, however, was re-esta-

blished, as soon as the pressure was removed again by creating a new vacuum around the organ. The injection was so perfect, that even several lymphatic glands in the vicinity of the transverse fissure, and which receive their afferent vessels from the liver, were beautifully injected. As the orifice of the canula, inserted into the hepatic duct, was only $\frac{1}{40}$ of an inch in diameter, the pressure exerted by the atmosphere and the gravity of the liquid must have been exceedingly small, which was furthermore indicated by the very slow descent of the liquid. I therefore hardly supposed that a rupture of the biliary tubules, and consequently an extravasation of the liquid into the parenchyma—by which the liquid might have entered the lymphatics—could have taken place. The last doubt, however, disappeared when I examined portions of the capsule from the surface of the liver under the microscope. Here, in some places, small portions of the parenchyma in which the network of "biliary tubules" was well injected with Prussian blue, were left. Arising from these, I observed a beautiful network of lymphatics, also injected blue, but more faintly than the "biliary tubules" (Fig. 5). When such a preparation is examined with an objective of $\frac{1}{4}$ of an inch focal length, the transition from the "biliary tubules" into the lymphatics can be distinctly seen. By the thinness of the capsule in this case, I was enabled to make even an examination with an objective of $\frac{1}{10}$ of an inch focal length. The lymphatics in the capsule on the surface of the liver of the sheep form a network of large capillary vessels, which converge to a number of centres where they join the larger vessels. Their larger size distinguishes them from the "biliary tubules."

To obtain still more conclusive proofs, I repeated the above experiments with some slight modifications, and the results I obtained in every instance were very satisfactory. I repeated the above experiment also with chrome yellow instead of Prussian blue; but as the former—consisting of fine granules—is not as fine as the latter, it required that the removal of the pressure of the atmosphere from the open lymphatics was carried to a greater extent, before the yellow colour appeared within them.

The phenomenon of a liquid, injected into the hepatic duct and returning by the lymphatics, has been noticed by most of the special investigators of the liver, as has already been mentioned. Without any further attempt to trace it to its source by microscopical examination, the majority of them were contented to attribute it to a rupture of the finest branches of the hepatic duct, causing an extravasation by which some of the finer lymphatics were ruptured, and an opportunity offered to the liquid to pass from one kind of vessels into the other. For my own part, I believe that extravasations do not occur as frequently as is generally supposed if the proper precautions are taken. They certainly take

place often if these are neglected. In the above-mentioned experiments on the liver of the sheep, the pressure employed was surely too little to rupture the "biliary tubules," and the exceedingly slow and gradual descent of the liquid would be sufficient reason to deny such an accident. The walls of the capillaries are very elastic, a fact which I have often observed, by stretching them to one-third more of their original length by means of the microscopic dissector.

To the best of my knowledge, Mr. Natilis Guillot is the only author who has asserted the existence of a natural communication between the hepatic ducts and lymphatics.

VI.—*A Microscopic Examination of the Atmosphere.* By George Sigerson, M.D., Ch.M., F.L.S., Member of the Royal Irish Academy.

I desire to lay before the readers of the 'Monthly Microscopical Journal' some of the results detailed in a lengthy paper which I read at the last meeting of the Royal Irish Academy [June 13], and which was entitled "Further Researches on the Atmosphere."

In a paper read at the January meeting I communicated the results of an examination of the "Sea Breeze," of "Country Air," and of "City Air," making reference to house-air likewise. Some question was made at the time with regard to the muriate of ammonia, which I described as found in combination with mucus in the city air. In order to have a faithful representation of its exquisite dendritic crystallization-forms, I had had photo-micrograms taken in June, 1869, but I did not find photography so accurate as the pencil. The chemical examination made by Dr. Angus Smith, by which he has found "albumoid ammonia" in the atmosphere, seems to corroborate my view.

In order to obtain an accurate knowledge of what is usually and what is occasionally present, it appeared requisite to make a careful examination of particular atmospheres as well as of the general air. A commencement was made in the previous paper, when the sea breeze, country air, and city air were investigated, as well as the more limitedly special air of our dwellings. The examination of special atmospheres has since engaged my attention. In the paper to which I have referred I detailed my mode of operation, and I may now proceed to describe the objects found in the sediment from the air taken from ledges above man's height, and observed likewise in the atmosphere.

And first of iron-factory air, a friable black dust—on examination this was found to be made up of particles of carbon, of ash, and of iron. The carbon formed the largest masses, the ash-particles were

reddish, or white and opaque. Some transparent pieces with a glassy fracture were noticed, and were regarded as glass resulting from the fusion of sand used in the welding process. An extremely fine powder—the dust of the dust—was sedulously examined for germs or spores, but it presented angular shapes and colours identical with the other matters. The particles varied in size, say, from $\frac{1}{50000}$th to $\frac{1}{500000}$th of an inch. A most interesting object was the iron, which was present in the form of balls. These were found to be hollow, and the fragments of their shells were discovered to be translucid: a granular structure was noticed in them. These iron bombs or balloons varied from $\frac{1}{500}$th of an inch to $\frac{1}{5000}$th, having been measured with the micrometer. The diameter of a bomb of $\frac{1}{2000}$th of an inch was computed to be sixteen times greater than the thickness of the shell, which would therefore be about the $\frac{1}{30000}$th of an inch. This computation was made in order to find at what degree of thinness its iron-material was translucent. [The iron was supposed to be present as magnetic oxide.] There were no spores nor seeds present, no fibres of any kind the result of developing germ-life—no fibres at all, except a few cotton filaments from the garments of artisans, fibrous carbon particles, and a specimen of contorted branchy metal. There were no germs here, yet the sunbeams were full of dancing motes, whose portraits were shown on the diagram. The ray, shining on them, assumed, in consequence of their hue, a bluish colour, similar to that observed when the carboniferous smoke of a candle or lamp is placed in the track of a sunbeam. On entering such an atmosphere, the taste of carbon, and indeed of iron, may be readily perceived. The dust might serve as a cheap stomachic for those who use charcoal biscuits or charcoal and bismuth powder.

Although a great quantity of this iron, carbon, and ash must daily pass in and out of the lungs; and besides, although a certain percentage must remain in them (as shown by Pouchet's dissections and Professor Tyndall's experiments), it was difficult to find a healthier body of men than those who work in such factories. One young man, whose lungs were weak, suffered from hæmoptysis (blood-spitting, with cough), which he had contracted in an American foundry where the heat was excessive. He was observed in an atmosphere murky with these motes, and was asked did it not affect him injuriously? He said, No; he found himself well in it; his cough came on at home on rising and lying down. These facts seem to indicate that the carbon poured into the air of cities, from gas-lights as from fires, may not have so injurious an effect as sometimes fancied, and may even tend to counteract the deleterious effects of some of the mucous particles. The prevention of black-smoke, whilst the injurious gases remained, might thus not be so exceeding great a service to the public health as popularly considered, although,

doubtless, a diminution of its excessive quantity could not fail to be a benefit.

The results of the study of the several other atmospheres, and their effects upon the health of those abiding in them, were described at length in my paper. A brief summary is all that can be given here. In "shirt-factory air," fine filaments and fragments of cotton and linen were found, with a few minute ova, not generally dispersed. These were translucent, and varied from $\frac{1}{750}$th of an inch to $\frac{1}{3000}$th of an inch, having been measured. The girls employed in the factory had become snuff-takers. In the atmosphere of "thrashing mills," fibres and fragments of chaff, awns, grain, together with some smut-balls, were found. In that of oatmeal mills, fibres were seen present in unexpected number, together with minute fragments of the pericarp of the caryopsis and starch-granules, with occasional spores and acari. This air was not so injurious as that of "flour mills," the dust being less. The atmosphere of mills where flax is "scutched" was found to be so bad that these mills could only be regarded as human slaughter-houses. Fine branchy filaments of the liber were present, with pointed particles of the hard, brittle wood-tissue; so that direct injury was done to the lung by these, and concretions formed by the filaments. The workers suffered much, and sanitary reformers and inspectors may be asked to adopt a plan whereby the men can be freed from this by causing the "stoor," or dust, to be partitioned off from them and sent out backwards. In the air of printing offices, from various symptoms observed, antimony was believed to be present. In consequence, some dust taken from a rafter 11 feet above the floor, was submitted to the secretary of the Royal Irish Academy, Professor W. K. Sullivan, whose analysis confirmed the accuracy of the expectation. Antimony was found, and no lead. In the "atmosphere of stables" were found moth-scales, some larva, eggs, spores, and a great quantity of cuticle-scales and fragments of fine hairs, with some corpuscles and fragments tinged blood-red. An acarus was present in the dust. Hairdressers had a similar atmosphere, so far as its distinctive elements, scales and hairs, were concerned. The "machine brush" increased their amount.

The next atmosphere was that of the dissecting hall. Fragments and fibres were found present, with the mark of the scalpel on them! There were fibrils of voluntary and of involuntary muscles, of white and yellow fibrous tissue; some epithelial scales, fat cells, corpuscles, fine fragments of hair, and inchoate particles. It was a somewhat ghastly revelation; but might be of advantage by inducing those in authority to ventilate thoroughly and disinfect daily.

Tobacco-smoke, with some difficulty, was got under the microscope. It was examined on entering and on leaving the mouth. Little globules of nicotine were discovered twirling and flitting

about in it, like monads. Some remained on the walls of the mouth; when the smoke is breathed (by novices), more globules are retained in the lungs, and nausea and illness supervene. These globules, if found in the air, distributed by a cigar smoker, might be taken for germs, as they would resist the iodine-test for amyloids.

In order to arrive at the cause of the ill-health of "tea-tasters," their special atmosphere was examined. In pursuing their avocation (selecting teas), they have to take a sip with quick inhalation, and thus a small shower of fine tea-drops enters their lungs. On examination of such tea-drops, a considerable quantity of fibro-vascular and cellular tissue, from the leaves, was found, which might aid to tease the lungs. But the real agents of mischief were numerous droplets of essential oil, very plentiful in Assam tea, which was particularly severe on the tea-tasters. Nausea, dis-arrangement of the nerves, and sometimes syncope afflicted them. The remedy would be to prevent the tea from entering the lungs; or, if they must take it, as at present, let them fill their lungs with pure air first, and breathe out immediately after tasting, to blow forth the globules.

As a result of numerous observations recorded in my paper, I consider myself justified in drawing the following conclusions:—

1st. That stomach signs—irritability, nausea, dyspepsia—are frequently symptomatic of interference with the lungs; so that to treat the stomach, in such cases, may obscure the symptoms, but will not cure the disease. The indication would rather seem to be to cleanse the lungs by inducing a mucous discharge.

2nd. That the lungs have a power, not hitherto suspected, of absorbing or assimilating even solid matter. It is clear, from what was stated in my previous paper, of Pouchet's anatomical investigations, as well as from Professor Tyndall's recent experiments, that a considerable quantity of matter frequently and generally remains in the lungs. The investigation into the health of iron-workers showed that the dust remained without causing injury. Unless the finer air-pipes are to be filled up by the daily deposit, this must somehow be disposed of by absorption. Old lungs are grey from the dust they take up. It may be remarked that carbonic dioxide acts as a solvent in many cases, and that we have in the lungs carbonic dioxide, oxygen, and moisture, all which favour the dissolving act.

3rd. That the theory of the panspermists seems unfounded on fact—that there are no hosts of germs *always* floating about in the atmosphere, invisible and maleficent as genii of Eastern stories. Air is not much better, but not generally worse than water. Professor Tyndall has set the sunbeam prominently before us; but I have come upon atmospheres, several times, where a sunbeam could

not be seen for want of motes. Within doors, at a country place, in winter and spring, the motes were plenty, and the sunbeam well defined. Out of doors, in a calm spot, I could see the bright slit where the ray entered the arbour. I could see the bright spot on the floor where it fell; but between these two visible points there was no visible line. It was impossible to say where the ray was, until the hand was placed in its way, or a little dust shaken through it. The dust then produced patches of light in the invisible ray, just as Professor Tyndall caused patches of stellar darkness by expelling the dust from the visible ray.

4th. The "germ theory" asserts "that epidemic diseases are due to germs which float in the atmosphere, enter the body, and produce disturbance by the development within the body of parasitic life." It is opposed to the opinion that epidemic diseases are propagated by a kind of malaria, which consists of organic matter in a state of *motor-decay*. It is supposed to be supported by certain statements about the non-occurrence of putrefactive changes in closed vessels, which have been disputed; and by the statement that rotten malaria cannot act like leaven, because fermentation is caused by the growth of the yeast-plant. Now, the effect of the growth of parasitic plants in causing skin diseases is well known; if they caused epidemics likewise, their presence would, in all probability, have been as soon detected. Pouchet, and recently Dr. Bastian, dispute Pasteur's views. The author of this paper, having opened the dense shell of a cocoa-nut, and cut through its oily albumen (both perfectly intact to all seeming), found in the milk a web-like plant, a kind of Achlya. I do not believe it probable that it got its "germs" from the air, but regard it as an example of the convertibility of matter—and correlation of beings—rather wrongly called by some spontaneous generation. [If it got its germs from the air, then far more readily could the air in the pleura get germs from the outer air, in Professor Lister's sample case. For if the dense tissues of the cocoa-nut coverings could not filter out the germs, neither could they be kept out of the pleura-cavities. Microphytes have been found in eggs, and mould in the brain of the golden pheasant.] Again, a dissection wound shows, in its results, how rotten matter may act with the quickness of leaven. However, this theory should be called the theory of *motor-change*, rather than of *motor-decay*. It yet remains to be proved that the facile explanation of fermentation by the growth of the yeast-plant is true—or being true, is the only true one. Two distinct agencies might cause an identical effect. Gay-Lussac and Liebig disbelieved in it. Pasteur, says Professor Tyndall, finally exploded their views of fermentation. Nevertheless, with very sincere respect for both these distinguished men, I must believe that the yeast-plant can have no retrospective action, that it cannot influence work done

before its birth. Now, before the wort is made, and the yeast-plant developed, molecular change has really already begun. Its effect is the difference between malt and raw grain. Starch has been altered into dextrine and sugar without the yeast-plant, which may next assist by its growth or decay (as some think) to continue the alteration until alcohol and vinegar result. In castor-oil and colza seeds, Frémy found that the fatty matter was converted into dextrine and sugar by fixation of oxygen, without, of course, the assistance of the yeast-plant. Payen and Persoz attributed such change to the action of diastase; later observers assert that it can be effected, under suitable circumstances, by any albumoids, they, themselves, being partially altered by the action of oxygen at a certain temperature, with moisture present. Now, in the mucus present in the atmosphere, as shown in my previous paper—in those granules resembling exudation granules, found plentifully in the atmosphere of a fever patient, and whose contagious influence was mentioned—in these, and such as these, the requisite albumoid substance is found. And I am convinced that it depends upon whether or not these or such albumoids be in a condition of motor-change, to determine whether or not we shall have present that contagious matter whereby certain diseases are engendered and communicated.

VII.—*The Microscopical Examination of Rocks and Minerals.*

By S. Allport, F.G.S.

The application of the microscope to Geology is a subject which has, I believe, been more generally neglected by men of science than any other of equal importance; although it is easy to show that this method of examination affords the readiest means of obtaining information on many important points, and is, in fact, the only way of ascertaining with certainty the composition of many compact or fine-grained rocks.

A valuable paper by Mr. Sorby, "On the Microscopical Structure of Crystals,"* and another by Mr. David Forbes, "On the Microscope in Geology," in the 'Popular Science Review.'† constitute, I believe, the extent of the published information on the subject. No branch of science presents a more promising field of inquiry, for the nomenclature and classification of igneous rocks is in a lamentable state of confusion, and unfortunately the maps of the Geological Survey of England and Wales tend rather to increase the difficulty by the indiscriminate use of terms not very definite in themselves.

It is evident that no satisfactory classification of rocks can be

* 'Jour. Geol. Soc.,' vol. xiv. † Vol. vi., p. 355.

made till their mineral constituents are accurately ascertained; and in the case of fine-grained or compact rocks, this cannot be done, either by chemical analysis, or other ordinary methods of observation. In such cases, various expedients have been adopted, but only approximative results were obtained, and that they were altogether unsatisfactory will be evident from the following remarks of Bischof.* "It appears," he says, "to be quite in vain to attempt a classification of Greenstones; they constitute a series, the extreme members of which are, on the one hand, Augitic Porphyry, or Melaphyr, with distinguishable Augite and Labradorite; and on the other Diorite or Syenite, with distinguishable Hornblende. Between these opposite extremes there are many intermediate varieties, with regard to which it is quite uncertain whether they are to be classed with augitic or hornblendic rocks." Again, Cotta † describes Aphanite, as "Trap in part, Melaphyr in part, a compact apparently homogeneous mass. The separate ingredients of this rock are not to be recognized by the naked eye, hence the name Aphanite given by Haüy. The minuteness and intimate union of the ingredients of Aphanite when quite compact, make it impossible with the ordinary aids to discover whether it belongs to Diabase, Gabbro, or Diorite." Now it appears to me that to call a rock Aphanite, or to say that a classification of Greenstones is impossible, is just as absurd as it would be for a zoologist or botanist to assert that no satisfactory account could be given of animal or vegetable tissues because they cannot be made out by the naked eye or a pocket lens. The examination of many rocks, if not too fine grained, may be facilitated by examining a polished surface with a low power, or even by simply wetting the freshly-broken surface instead of polishing; but in the case of basaltic and other fine-grained rocks it is necessary to prepare thin sections for examination by transmitted light. This method is in fact by far the most satisfactory in the majority of cases; for not only may the various constituent minerals be readily observed, but the order in which they crystallized out from the mass, the changes which they have frequently undergone during the long lapse of ages, and other important facts, may also be thus ascertained.

A microscopical examination of rocks shows that, as a rule, the igneous rocks may be distinguished at once from all others by their structure, which is that of a more or less perfect network of minute crystals: in many cases all the minerals are well crystallized; in others there is an amorphous or glassy base in which they are enclosed; there are, however, other rocks, such as the Felstones and the more recent volcanic Phonolites some of which do not present this crystallized arrangement of their constituents; and then there are the Porphyrites, which are characterized by the presence of

* 'Chemical Geology,' vol. iii., p. 300. † 'Rocks Classified,' p. 157.

crystals of felspar in a compact felspathic base. There is in fact a gradual passage from the compact Felstones to the Porphyrites; so that it appears probable that the amorphous base of all such rocks is simply the silicious magma or paste from which distinct minerals would have separated had the circumstances under which they were formed been favourable for crystallization to have taken place. As a typical example of a widely-distributed class of rocks, I will take for description the well-known basaltic rock of the Rowley Hills. An examination of thin sections shows that it contains a triclinic felspar, augite, magnetic oxide of iron, a little olivine, and a few crystals of apatite. The felspar is known to belong to the triclinic system, as it exhibits the characteristic striæ when examined by polarized light. Augite occurs in minute black shining crystals, which appear bright brown, or occasionally green when in thin sections; it cannot be mistaken for any other mineral except hornblende, from which it is distinguished by a marked difference in the angles; it has also a clear glassy appearance, while hornblende is either distinctly fibrous in texture, or exhibits lines or cracks running parallel with the principal cleavage plane: very frequently hornblende is green, while augite is yellowish brown, but this does not always hold good. The magnetite occurs in minute grains, or as thin laminæ, both being black and opaque. The apatite is seen in long acicular hexagonal crystals. The olivine occurs in crystals, and also in irregular grains, of a clear yellowish green colour; it is, however, rare in this state, being nearly always altered to a dull dark-green mineral. In addition to the above minerals, which must be regarded as the original constituents of the rock, there are also one or two zeolites, calcite, and a chloritic mineral, all of which are found filling cavities, and are secondary formations.

The use of the microscope is by no means confined to the discrimination of minerals; with its assistance we may learn many facts, as to the mode of formation of rocks, the order in which the various minerals crystallized, and the alterations which have been frequently caused by the removal of mineral matter, and its replacement by another of different chemical composition. In the Rowley rock, the minute crystals of apatite penetrate both the felspar and augite; the latter also encloses crystals of felspar and magnetite: the augite crystallized therefore after the others had been formed. The olivine contains grains of magnetite only, and was probably the second to crystallize.

Cases are not uncommon in which crystals have caught up portions of the surrounding mass while in the act of formation, and other facts indicate very clearly the actual condition of the mass at the moment of crystallization. For example, in a section of Pitchstone from Planitz, containing crystals of felspar, the minute opaque

particles thickly scattered through the matrix are crowded together round the sides of the crystals, having been forced outwards as the latter increased in size; this clearly indicates that during the formation of the crystals, the matrix was in a *viscid*, but not in a *fluid* state, for had the particles been quite free to move, there would have been no crowding.

In a section of basalt from the Rhine, the olivine is in its usual fractured condition, and some of the larger cracks have been filled up with the fine crystallized matrix in which they are imbedded;—there is no crowding of the particles; in this case, therefore, the olivine was not only crystallized, but fractured, before the consolidation of the mass. In another section of basalt the crystals of augite and olivine are somewhat rounded, and the cracks filled up, so that they probably existed as crystals or grains before the ejection of the lava.

One of the most important aids in the examination of rocks and minerals is afforded by polarized light. In many cases it enables us at once to discriminate between different minerals, and not unfrequently affords clear evidence of changes which have taken place subsequently to the consolidation of the substance under examination. When a thin section of a crystal is placed on the stage of the microscope, and a beam of polarized light is passed through it, the beam is depolarized, and generally exhibits colours due to interference; the intensity of colour varies according to the direction in which the crystal is cut, and consequently, in examining a section of rock, the various sections of any one mineral do not always give the same result; but as the crystals of igneous rocks lie at all angles, it is nearly always easy to obtain some which, being cut at an inclination to the optic axis, exhibit different degrees of intensity of action; therefore minerals which vary much from each other in this respect may be easily distinguished.

A most important point to be noted is, that the depolarizing action of a crystal is uniform over the whole surface of its section, if it consists of one simple crystalline structure; when, however, the light appears to break up into detached parts, each of which changes independently as the analyzer is rotated, we know that it is made up of a number of separate crystalline portions, either independent of each other, or sometimes related as twins.*

A knowledge of these facts enabled me to detect the presence of olivine and its pseudomorphs in the Rowley rock, as described in a former paper, and also in the 'Geological Magazine,' vol. vi., p. 115. A pseudomorph is a mineral possessing a crystalline form, which does not belong to the substance of which it is composed; it is an altered mineral, or, in other words, an aggregate of mineral matter,

* Sorby "On the Examination of Rocks and Minerals," in Dr. Beale's work on the Microscope.

which has been deposited simultaneously with the removal of that which possessed the original crystal form; it is easy, therefore, to see that the molecular arrangement of the particles must be entirely different from that of the original crystal. Now, by the aid of polarized light, such changes are at once rendered apparent, and we thus possess the means of obtaining most important information on the metamorphism of rocks and minerals, of which ordinary light would afford no indication whatever.

Serpentine has hitherto been a great puzzle to geologists, some having regarded it as an intrusive igneous rock, others as of metamorphic origin. As not unfrequently happens, both are, I believe, right, for every section I have made clearly proves it to be an altered rock, and one specimen from the Vosges mountains contains numerous grains of olivine, in which the change is only partially developed.

These few facts will serve to indicate the importance of this hitherto neglected method of inquiry; for although the pseudomorphism of many minerals has been long studied, little attention has been directed to similar changes in rock masses.

A subject of interest to the microscopical observer, and one of considerable importance to the petrologist, is the occurrence of minute fluid cavities in the minerals of igneous and metamorphic rocks; they have been detected in several minerals ejected from active volcanoes; but so far as I have observed, they are far more abundant in quartz than in any other mineral. Those who wish to examine them may do so by making a section of almost any specimen of granite; they are very numerous in the granites and schorl rocks of Cornwall, the hornblendic granite of Mount Sorrel, in the syenite and gneissoid rocks of Malvern, and in the syenite of Croft Hill, and neighbouring bosses in Leicestershire. In these and similar rocks, the fluid cavities appear to be so entirely restricted to the quartz, that I have not yet detected any in the felspar or mica; they are certainly extremely rare in these minerals, if they occur at all; this, if established, would indicate a difference in the condition under which the minerals were formed, a point which I believe has not yet received attention.

For an account of the curious spontaneous movements of the fluid in some of these cavities, and for other interesting matter connected with the subject, I must refer you to Mr. Sorby's paper already quoted.

During the past summer and autumn (1869) I have collected specimens of the igneous rocks of the Midland coal-fields from the following localities:—Kinlet and Shatterford, west of Kidderminster; the Clee Hills; Little Wenlock, near the Wrekin, in Shropshire; Coalville, near Bardon Hill, in Leicestershire; and Matlock, in Derbyshire.

A microscopical examination of thin sections shows that all these rocks belong to the same type; they do not in fact differ more from each other than do different specimens of any one of them. The Toadstone of Derbyshire is merely an amygdaloidal variety.

The rocks of the Warwickshire coal-field differ considerably from the foregoing; they contain hornblende instead of augite, and are therefore true greenstones or diorites; they may be readily examined in the railway cutting near Nuneaton, and also a little to the west of Atherstone. All the rocks just enumerated are clearly older than the surrounding Permians, which are never penetrated by them.

Having now made upwards of four hundred sections of rocks and minerals, I am inclined to believe that the following results of microscopical examination will stand the test of further study.

1. The mineral constituents of the melaphyres and other fine-grained igneous rocks may be determined with certainty—a result which has not been attained by any other method of examination.

2. The mineral constituents of the true volcanic rocks, and those of the old melaphyres, are generally the same.

3. The old rocks have almost invariably undergone a considerable amount of alteration, and this change alone constitutes the difference now existing between them and the more recent volcanic basalts.

The basaltic lavas of the Rhine and Central France are composed of a triclinic felspar, augite, magnetite, olivine, and frequently apatite, the same minerals as those constituting the old rocks above described. I have fine-grained specimens of the latter hardly distinguishable from recent basalts; and a section of Dolerite from the Puy de Barnère, in Auvergne, does not differ in any important particular from coarse-grained specimens from Rowley.

It would be easy to extend the parallelism to other classes of rocks, but I will now only observe that we have here another proof of the doctrine long taught by Lyell—the uniformity and continuity of the Laws of Nature.

[The foregoing important contribution is taken from the 'Proceedings of the Birmingham Natural History and Microscopical Society,' a most interesting volume, just published.—Ed. M. M. J.]

NEW BOOKS, WITH SHORT NOTICES.

On Microscopical Manipulation; being the subject-matter of a Course of Lectures delivered before the Quekett Microscopical Club, January–April, 1869. By W. T. Suffolk, F.R.M.S. London: Gillman. 1870.—The lectures on microscopic manipulation, which Mr. Suffolk last year delivered before the members of the Quekett Club, appeared successively in the pages of the 'Chemical News.' From the latter journal they have been reprinted by the author, and are now issued in the form of a very prettily-bound volume. We presume that the author does not intend his book to enter into competition with the larger treatises of Carpenter, Beale, and Hogg, but that he has written for those who may be desirous of reading something short and introductory before commencing the study of a standard work. If this book were to be regarded as in any way a comprehensive guide to the microscopist, it must meet with severe criticism. As an elementary volume it is creditable alike to Mr. Suffolk and the Quekett Club, and it will be found useful by amateurs who have just purchased their first microscope. It contains seven chapters, an appendix, and some good notes. The titles of the several sections give a key to the contents, and therefore we reproduce them as follows:—Construction of Microscope; Mechanical Processes; Mounting Objects Dry and in Balsam; Mounting Objects in Fluid; Illuminating Apparatus; Polarized Light; and lastly, Drawing and Micrometry. There are forty or fifty woodcuts scattered through the text, and there are eight lithographic plates on a black background. In most of the chapters we find but an outlinear summary of the facts stated in Carpenter's 'Microscope,' but in some, as, for example, the second, we are given an amount of valuable practical information, the result of the author's extensive experience. Mr. Suffolk's remarks, too, on the method of drawing and lithographing sketches of microscopic objects are exceedingly good. The author is but an amateur lithographer himself, but some of his plates—especially that of *Stephanoceros*—give promise of considerable ultimate skill. It seems to us that Mr. Suffolk might have omitted the consideration of such questions as the reason why a convex lens when placed before the eye magnifies an object. This is a difficult problem indeed, and Mr. Suffolk has allowed to escape him a very important fact in connection with the subject. The magnifying power of such a lens is not due simply to its rendering more convergent or less divergent the rays from an object placed *within* the range of normal vision. As Dr. Carpenter (and we fancy he is the only writer who has done so) has well pointed out when treating on this subject, "Not only is the course of the several rays in each pencil altered as regards the rest by this refracting process, *but the course of the pencils themselves is changed, so that they enter the eye*

under an angle corresponding with that at which they would have arrived from a larger object situated at a greater distance." * We doubt not that Mr. Suffolk is familiar enough with this fact, but what we complain of is that he does not express it with sufficient clearness. The volume is one, nevertheless, which will deservedly attain popularity among amateurs commencing microscopic inquiries.

Proceedings of the Birmingham Natural History and Microscopical Society during the year 1869. Part I. Birmingham: Sackett & Edmonds. 1870.—The Natural History Society of Birmingham has certainly been successful, even in its first effort, and has issued a capital volume of 'Proceedings' most creditable as a whole to its members. Perhaps the highest proof of our good opinion of the work is the fact that the Editor has selected one of its papers for publication in these pages. The illustrations are numerous, and in most cases good; those of Mr. Wills on microscopic crystals, and of Mr. Marshall's on the transformations of the Gnat, being especially well-executed and scientific representations of objects seen under the microscope. We must not criticize too closely the several contributions to the first volume of a provincial Society's 'Proceedings,' but we would remark that a few of the papers are so flagrantly mere compilations from the most ordinary text-books they should not have been allowed to be read before the Society, and are certainly not worth the time expended by the compositor in setting them up. We are sorry to observe, too, that these papers are generally by persons whose titles and position entitle them to be regarded as belonging to the scientific world. We doubt not that "a word to the wise" will be sufficient, and that the many able and distinguished original workers which the Society embraces will soon weed out, or at least convert into listeners, those members from whom better things should be expected, but who palm off on a body of earnest labourers, papers which are no more valuable or original than a schoolboy's essay. It must be said that contributions of this kind are the exception, not the rule, and that as a whole the Birmingham Society's first volume is a really good and useful one.

PROGRESS OF MICROSCOPICAL SCIENCE.

Structure of Wood from the Cairo Petrified Forest.—In the 'Geological Magazine' for July, Mr. Carruthers publishes a very interesting paper, illustrated by a capital plate, describing the results of his examination of the wood from the curious petrified forest of Cairo. After a full account of the general and geological features of the fossil wood of the desert, he goes on to state its peculiarity of struc-

* 'The Microscope and its Revelations,' 4th edition, p. 19. The italics are ours.—REVIEWER.

ture. The wood, he says, is converted into chalcedony. The tissues are, on the whole, not well preserved, although occasionally specimens occur in which every cell and vessel is clearly defined. Unger detected in some specimens the branching mycelium of a parasitic fungus, penetrating the cavities of the large ducts, to which he gave the name of *Nyctomyces entoxylinus*.* In none of the large series of microscopic preparations which Mr. Carruthers examined (21 from the specimens collected by Prof. Owen, two from the Bryson Collection, and two from that of Robert Brown) was he able to detect this fungus. In all of them the ducts are filled with transparent chalcedony, which occasionally shows a dark, amorphous and irregular, sometimes branching core, passing down the centre. The chalcedony is more or less filled with minute bodies resembling transverse hexagonal prisms of quartz; but they are thin plates or cavities penetrating the chalcedony in every direction. They have an amorphous centre, and exhibit concentric lines of growth. They do not affect the light differently from the substance in which they are imbedded, when examined by the polariscope. Newbold refers to a specimen shown him by M. Linant, at Cairo, which had apparently a somewhat similar structure, but Mr. Carruthers thinks it must have been on a much larger scale, as he does not speak of using any magnifying power in examining it. He says it "had the hollow lined with a white chalcedony-like silicious substance, full of small cells resembling those of a honeycomb."† The stems, as determined by Robert Brown, and confirmed by Unger, belong to Angiospermatous dicotyledons, and are made up of the tissues that enter into the composition of such plants. The wood consists of slender prosenchyma, abundantly penetrated by large ducts, which occur singly or arranged, two, three, or even more together. The walls of the ducts are marked with small, regularly-arranged oval, or somewhat compressed hexagonal, reticulations. Transverse diaphragms break up the ducts in well-preserved specimens into oblong compartments about twice as long as broad. The medullary rays are abundant, and form a considerable proportion of the stem. The concentric layers of wood are not so well defined as one generally finds them in dicotyledonous stems, because of the irregular manner in which the ducts occur throughout the year's growth. In this respect they resemble the walnut rather than the oak. Mr. Carruthers remarks that the structure of stems have not been hitherto sufficiently regarded by systematic botanists, and recommends that attention be more directed to this subject. In conclusion he says that "owing to this he cannot go beyond R. Brown in saying that these stems are dicotyledonous, they are not coniferous."

The Metamorphoses of the Gnat.—Mr. Marshall communicates to the 'Proceedings of the Birmingham Natural History Society' a nice paper on this subject. He gives two plates showing the successive transformations of the larva. The powers employed are very low, as the object of the transformations within the ovum have not been studied.

* 'Chloris Protogæa,' p. 8, Tab. I., Fig. 7.

† 'Quart. Journ. of Geol. Soc.,' vol. iv., p. 353.

Are the Pus-corpuscles derived from the White Blood Cells?—There is no question in microscopic anatomy which has attracted so much attention from physiologists as this during the past two or three years, and now it seems, according to the researches of M. Picot recently laid before the French Academy,* June 20, 1870, that the idea of Conheim that the pus-corpuscles are partly produced by the passage of the white blood cells through the blood vessels is altogether a mistake, is a misinterpretation of the phenomena in point. M. Picot, whose memoir was presented by M. Robin, gave a tolerably long account of his observations on the circulation of frogs and mammals, and he declares most positively that the white blood cells never pass through the vascular walls, and that the pus-cells are formed gradually, external to the capillaries. He explains the error of Conheim and others, by stating that they confounded several focal planes together, and he considers that he has demonstrated this in the following way. He counted the number of white blood cells in the arrested blood in the capillaries, both before and after the quasi-exuded corpuscles appeared. In both instances he says the numbers were the same, and this could not have been if the white cells had passed outwards.

A New Peronospora, Parasitic on Cactus.—In a paper read before the French Academy on the 13th of June, MM. Lebert and Cohn, of Breslau, describe a new species of *Peronospora* infecting the Cactus. Several fine specimens of Cactus were seen to putrefy and decay while the external surface seemed healthy. A section through the cellular tissue showed it to be completely disorganized; the cell-walls being quite broken down and the masses of crystals and starch-granules being immediately set free upon a glass slide. At the same time there was spread throughout the mass a vast quantity of mycelium. This latter was in the form of unicellular threads, very long and thin, undulating in their course, of unequal calibre throughout, and filled throughout with colourless vesicles or droplets. They divided nearly at right angles and at irregular distances. It was seen on careful examination that the mycelium had not originally traversed the substance of the cells, but had spread around and between them, thus forming an elaborate mesh-work. The author, having given a long account of the fungus, called it *Peronospora cactorum*.

Amœbæ and Monads.—The 'Proceedings of the Bristol Naturalists' Society,' † just issued, contains a paper on these forms, by Dr. Henry Fripp. The paper is a long one, and was some time since reported in full in 'Scientific Opinion,' and we need not therefore abstract it. But we commend it to the careful study of our readers. It contains some original matter, and a careful survey of the work of English and Continental workers.

Ciliary Movements and their Nature.—Mr. N. E. Green has been for some time engaged in observations on Rotifera and other microscopic animals, with a view to discover the exact character of the action of the cilia. In a paper on the subject, which is published by him in the 'Journal of the Quekett Club' for July, he describes

* 'Comptes Rendus,' June 20th. † Vol. iv.

his several observations on Rotifera. Many very interesting and novel facts are recorded, but the nature of the ciliary movement is still practically left unsolved.

New Mode of Sub-stage Illumination.—Dr. J. Matthews has devised a plan for this purpose which has been very favourably spoken of. It consists in the employment of a low-power object-glass so fitted that the pencils proceeding from it may be thrown at any angle on the object. Dr. Matthews has solved the difficulty of sub-stage illumination by adopting one of the objectives themselves, of lower power, as an illuminator in place of a condenser, but not axially. It must be so mounted as to send the whole of its bundle of rays at angles varying with the requirements of any given case, and in this consists the value of the method. Any of the powers may be used, bearing in mind that the higher the examining, the lower, within convenient limits, should be the illuminating power, in order to secure a proportionate amount of light. The only use of condensers of short foci and wide angle is to get the more oblique rays of the cone by stopping out some part of the rest. Dr. Matthews' best results have been procured by a $\frac{2}{3}$rds or $1\frac{1}{2}$ object-glass, which gives in all cases quite sufficient light. There is no limit for the angle at which the illuminator may be used in relation to the axis of the instrument short of 90°, supposing both the covering and the mounting glass as well as the stage to be of no thickness; but as they all have a very sensible one, and it is found that rays of a greater angle than about 83° do not pass through the slide to the object, but are reflected and lost, he has found it better to work at angles varying from 25° to 65°.—See *Journal of Quekett Club*, July.

How Motor-nerves end in Non-striated Muscular Tissue.—A very valuable communication stating the results of M. Hénocque's researches has been published in the 'Archives de Physiologie' (May), and may be thus abstracted:—1. The distribution of the nerves in smooth muscle is not only identical in man and other vertebrate animals in which it has been observed, but is essentially similar in all the organs containing smooth muscle. 2. Before terminating in the smooth muscles, the nerves form three distinct plexuses or networks—(*a*) a chief or fundamental plexus, containing numerous ganglia, and situated *outside* the smooth muscle; (*b*) an intermediate plexus; and (*c*) an intra-muscular plexus, situated *within* the fasciculi of smooth fibres. 3. The terminal fibrils are everywhere identical; they divide and subdivide dichotomously, or anastomose, and terminate by a slight swelling or knob, or in a punctiform manner. The terminal swelling appears to occupy different parts of the smooth muscular fibre, but most frequently to be in the neighbourhood of the nucleus, or at the surface of the fibres, or, lastly, between them. The methods of investigation adopted by M. Hénocque have been very numerous, including the maceration of the preparations, obtained in as fresh a condition as possible, in aqueous humour, artificial serum, pyroligneous acid, chromic acid, chloride of gold and potassium, especially the latter, in a strength of $\frac{1}{200}$th.

Action of Light on Fertilized Frog's Ova.—Herr Auerbach recently contributed to certain numbers of the 'Centralblatt' the results of his observations. According to the 'Lancet,' which abstracts his conclusions, he states that during the spring (April 13th to 20th) of the present year he had frequent opportunities of observing the spawn of the frog, and was particularly struck by the circumstance that even the diffused light of day, but especially the direct light of the sun, constituted a powerful excitant of the contractions of the protoplasm of the egg. If the amount of light falling upon an egg, or, more correctly speaking, on the primary segmentation spheres and the secondary results of fission, be increased, alterations of form may be seen to occur even under the eye of the observer. If the egg be so placed that the white pole is directed towards the light, the contractions excited have a tendency to push the black pigment in part over the white area, or even to completely cover it, whilst the opposite pole becomes whiter. When the first meridianal furrow has been completed, this pigmentation of the clear area occurs to a less extent. This alteration in the distribution of the pigment must not be confounded with the well-known rotation of the yelk, which depends on the centre of gravity being near the white pole.

NOTES AND MEMORANDA.

Medico-Legal Microscopy Extraordinary!—Some evidence recently given in a case of murder, tried in America, and which relates to the value of the microscope in medico-legal inquiries, seems to us so extraordinary, and indeed so nearly impossible of belief, that we shall lay the facts before our readers with a hope to elicit their opinion. The case was that of "The People *v.* Elisha B. Fero," who was tried for the murder of his wife at the Delaware (U.S.) Assizes in September and October last. Among other important and interesting points in this remarkable case, the question arose as to whether certain injuries of the head had resulted from violent bruises inflicted on the head. In reference to this, two experts, Drs. Van Derveer and Stevens, stated that they formed the opinion that the head was bruised (1) by the appearance of the tissues; (2) by cutting into them; and (3) by a microscopic examination of them. Now it was from this last method especially that they formed these conclusions; and as the whole subject has been very cleverly handled by Dr. Charles H. Porter in a critical article in the (American) 'Journal of Psychological Medicine,'* we cannot do better than reproduce the writer's facts and comments on the subject, even though they extend to some length, as one of the practical applications of the microscope is of the highest importance.

Let us briefly consider the nature of these examinations, which

* Vol. iv., No. 2.

were so satisfactory to these experts that they were willing to deliberately swear that the tissues of Mrs. Fero's head were bruised.

Both Dr. Stevens and Dr. Van Derveer state that they saw broken muscular fibres in certain specimens of tissues from Mrs. Fero's head, which they examined under the microscope with powers magnifying objects from 700 to 1000 diameters. We do not question the accuracy of their observations in this particular; but of what importance is it in determining whether or not the tissues were bruised, whether blows were struck before death?

According to Bowman, the average diameter of the ultimate muscular fibres of the human female is $\frac{1}{454}$th part of an inch; these are bound together by connective or cellular tissue into small bundles, and these again into larger ones, which constitute muscle or lean flesh. In order to examine these ultimate muscular fibres the usual method is to take a small portion of a muscle as fresh as possible, but after it has lost its contractility, and, using magnifying glasses, the particle is torn under water into fine shreds by means of needles. The connective tissue is so strong that force must be used to separate the ultimate fibres, which as a consequence are in parts irregularly broken or torn. The specimens thus prepared may then be submitted to examination under a high power of the microscope, in such a condition as to exhibit most of the important points in their structure.

In this case, however, broken muscular fibres were not found in *fresh* specimens from Mrs. Fero's head, prepared by the above or similar manipulations, but only in specimens of the tissue after it had been subjected to the action of strong alcohol five months. But, as has just been said, the above method of preparing fibres for examination is very certain to break them. Even were this not necessarily so, would it be safe or wise to say that the delicate muscular fibres were not broken during these manipulations, when the bulk of the material worked upon in each case was much smaller than the head of a pin? But this is by no means all: it is necessary to bear in mind the treatment the head had undergone before these examinations. It had been subjected to the action of saws, knives, and fingers, during two *post-mortem* examinations; to burial and exhumation; to repeated handling in Davenport, Delhi, and Albany; it had been placed on wooden tables and marble slabs, and the muscular parts had been cut, pulled, and separated from each other at different times and places. It would be strange, indeed, if thousands of fibres were not broken during these operations, as undoubtedly they were—this even a casual examination would show—and stranger still if broken fibres were not detected when the tissue was examined under the microscope. Both of these experts, on their cross-examination, admitted that they could not determine by the microscope whether the fibres were broken before or after death; we agree with them in this, and hence fail to see how their discovery of broken fibres, under the actual circumstances, was the slightest evidence of blows or bruises.

Dr. Stevens saw not only broken muscular fibres, but also some other objects which he thought might be fibrin. Let us examine into this. The blood contains, besides water, blood-globules, albumen, and

fibrin. The coagulation of blood is caused by the spontaneous solidification of the fibrin, as a network of filaments, which entangles the blood-globules, and forms a mass called the clot. The albumen remains liquid, and is gradually forced out as the clot contracts. If blood is placed in alcohol, the albumen solidifies in minute granules. The albumen being thirty to forty times as abundant as the fibrin, when both of these constituents are solidified, which takes place when blood or a tissue containing blood is placed in alcohol, the filaments of fibrin cannot be distinguished, as they are concealed by the albuminous granules. The same condition was induced when Mrs. Fero's head was immersed in alcohol. Although the blood had coagulated, the fibres were surrounded by a liquid containing albumen, which soon formed granules, and concealed the fibrin-filaments. Hence it seems unreasonable to believe that Dr. Stevens saw fibrin, as he thought probable. The following question was proposed to the same gentleman (a question which, properly answered, was regarded by the defence as of great importance): "Can you distinguish between a piece of flesh stained only with liquid blood, then soaked in alcohol, and afterwards bruised, and a piece of flesh bruised before death, and then soaked in alcohol?" He replied that he could not distinguish between them.

Dr. Van Derveer found in the specimens examined by him broken ultimate muscular fibres, also some healthy muscular fibres. He likewise stated that he observed *fragments of broken blood-globules driven into the ultimate muscular fibres*. This statement is of such a character that its mere announcement might be regarded as its sufficient refutation; but, lest injustice be done to the expert who made it, it is proper to submit it to examination. The blood-globules of man have an average diameter of $\frac{1}{3000}$th of an inch. They are not solid bodies, but, on the contrary, have a consistency which is very nearly fluid. They are homogeneous, and are not provided with an investing membrane. It is these minute, almost fluid bodies, we are told, which were shattered by blows upon the head, and their fragments—fluid be it remembered—forcibly driven into the solid ultimate muscular fibres, the average diameter of which is $\frac{1}{454}$th part of an inch. And this observation, it should be borne in mind, was made not upon fresh blood-globules, but upon the alleged fragments of blood-globules which had been subjected to a five months' immersion in ninety-five per cent. alcohol.

It appears that, after this long immersion of the tissue in alcohol, Dr. Van Derveer swears that he positively identified, *as fragments of blood-globules, certain particles which he saw in specimens of the tissue examined under the microscope! Moreover, he swears that he saw these fragments of blood-globules (fragments of fluid bodies) imbedded, like wedges in a log of wood, in the substance of the solid ultimate muscular fibres, where, we are asked to believe, they had been mechanically driven five months before, by the force of blows which were assumed to have been struck upon the head of Mrs. Fero.* But, even if we admitted the correctness of the observation, and that blows might produce such effects, of what importance is it in the case, when we are unable to determine whether the condition described was produced before or after death?

Photographs of Nobert's Lines.—Dr. Woodward, of the United States' Army Medical Department, has just presented to the Royal Microscopical Society and to a few private friends, four admirable photographs of the above. The first represents the 1st, 2nd, 3rd, and 4th bands of the 19 band-plate, magnified 1200 diameters by Powell and Lealand's $\frac{1}{16}$th immersion. The second represents the 8th, 9th, 10th, and 11th bands of the same plate, magnified 1100 diameters with the same objective. The third displays the 13th, 14th, and 15th bands of the same plate, magnified 1100 diameters with the same objective; and the fourth contains two photos, one a large one representing the 19th band of the 19 plate-band, magnified 1100 with Powell and Lealand's $\frac{1}{16}$th immersion, and the smaller an enlargement of part of the other to 2750 diameters.

The Presidentship of the Quekett Club.—It is arranged that Dr. Lionel Beale, F.R.S., is to be the next President of the Quekett Club. It is needless to point out what an excellent selection the club has made, and we have no doubt that it will tend to strengthen that *entente cordiale* which is now so thoroughly established between the Quekett Club and the Royal Microscopical Society.

Influence of Light on the Cells of Spirogyra.—At the meeting of the Academy of Sciences on the 11th of July, the Montyon prize for Experimental Physiology was awarded to M. Famitzin for his researches, which have determined the precise action of light on the increase of the cells of *Spirogyra*, and on the development of the green matter.

Möller's Test-slide and the Navicula Lyra Photographed.—As illustrative of photo-micrography with the oxy-calcium light, a subject reported on by Col. Woodward, of the U. S. army, in our present number, the author has sent to the Editor and to the Royal Microscopical Society exquisite photographs of the above. The representation of *Navicula* is about 4 inches long, and is wonderfully well defined. That of Möller's curious collection of Diatomaceæ is not so good, owing to some of the specimens appearing simply as black disks. The *Navicula* is magnified 1000 diameters with Powell and Lealand's $\frac{1}{16}$th immersion; the test-slide is magnified 35 diameters, with one of Wales' $1\frac{1}{2}$-inch object-glasses.

French Prizes for Microscopical Researches.—At the meeting of the French Academy of Sciences on July 11th, the Desmazières prize was equally divided between MM. Hoffman and Rabenhörst; the first, author of a memoir on Bacteria, and the second for his work 'Flora Europæa Algarum aquæ dulcis et Submarinæ.' At the same time "honourable mention" was accorded to M. Edouard Strasburger for two memoirs "On the Sexual Organs and the Fecundation of Ferns."

Blankley's Universal Revolving Stage.—We give an illustration of Blankley's Universal Revolving Stage, a description of which was read before the Royal Microscopical Society, and appeared in this Journal,* so that it is only necessary to give an explanation of it here.

* Vol. iii., p. 209.

Fig. 1 shows the stage itself with a live box (which can be used with the spot lens) placed in the centre; and by rotating the milled disk shown at the edge, the object can be viewed at every angle of

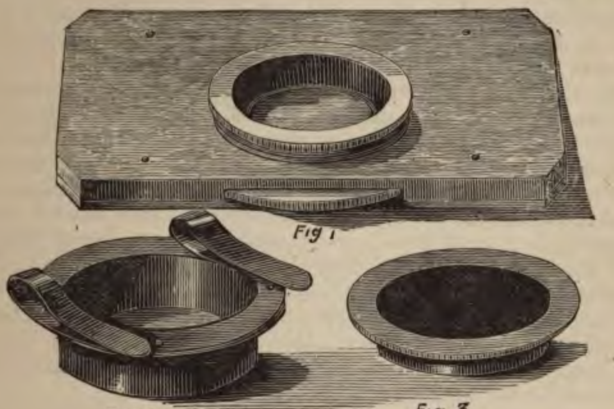
Fig 1 Fig 2 Fig 3

light. Fig. 2 is the mounted object-holder for transparent or opaque slides, and fits into Fig. 1. Fig. 3 is a cork disk for pinning on any object that may be wished to be seen in various positions.

CORRESPONDENCE.

The Definition of Nobert's Lines.

To the Editor of the 'Monthly Microscopical Journal.'

Washington, D.C., *June* 18, 1870.

Dear Sir,—The May number of the Journal contains on page 257 *et seq.* a letter from Mr. Charles Stodder, of Boston, in which that gentleman reasserts the claim made in his paper published in the 'American Naturalist' of April, 1868, that he and Mr. Greenleaf had resolved the 19th band of the Nobert's plate with an immersion $\frac{1}{8}$th, made by Mr. Tolles, of the Boston Optical Works, and alludes to me in a manner which seems to require some reply. His letter is followed, in the same number, by one from Mr. R. C. Greenleaf, in which that gentleman states his agreement with Mr. Stodder's opinions, and talks about "the honor of our American instruments."

Mr. Stodder writes with a warmth which will be best understood when it is known that he is the treasurer and agent of the Boston Optical Works, the establishment at which Mr. Tolles produces his really very excellent lenses, and that he has for some time claimed

that Mr. Tolles produces the very best lenses in the world. I am not willing to yield to either of these gentlemen in the disinterestedness of my desire for the success of American opticians, but am of the opinion that our progress will be hindered rather than helped if we shut our eyes to the few cases in which English or other manufacturers excel ours. In fact, where this is the case a recognition of the reality appears to me necessary to our progress.

Mr. Stodder's complaint that his paper in the 'American Naturalist' has been ignored, certainly cannot apply to me. In my first essay on the Nobert's plate,* I quoted from it, and gave the reasons why I thought Messrs. Stodder and Greenleaf had been misled by spurious lines. At that time I had not been able to resolve any band beyond the 15th, and I did not succeed with the 19th until I obtained the immersion $\frac{1}{16}$th of Powell and Lealand in the spring of 1869. So that if Messrs. Stodder and Greenleaf's original claims were really well founded, they certainly ante-dated mine. Nevertheless, I still think they were at that time mistaken, and do not now believe that the $\frac{1}{10}$th, with which they at present claim to have resolved the lines, has really succeeded in doing so. I will endeavour to state very briefly my reasons for this opinion.

In my judgment it cannot be said that one of the bands of the Nobert's plate is resolved unless the lines are shown in such a manner that they can be correctly counted from one edge of the band to the other. Objectives of inadequate defining power can easily be made to show lines in the higher bands which would deceive even an experienced eye, and the lower the power the more readily the observer will be misled; but if such spurious lines are counted, they will be found too few in number, and their nature is thus shown. Now, not only did Mr. Stodder in his original paper admit that he had not counted the lines, but he fell into a grave error on the subject of counting fine lines, which he expressed in the following words :—"In counting lines of such exquisite fineness either the micrometer or the stage must be moved, and it is next to impossible to construct apparatus that can be moved at once the $\frac{1}{100000}$th part of an inch and no more." In consequence of this error, Mr. Stodder had omitted the only reliable mode of demonstrating that he had not been misled. My paper led to some correspondence between him and myself, in the course of which I endeavoured to borrow the lens in question for the purpose of testing it. I then learned that it had been unfortunately broken. Other $\frac{1}{5}$ths and $\frac{1}{6}$ths, made by Mr. Tolles, which from time to time I had an opportunity of testing, did not in my opinion justify the claims made. In the latter part of May, 1869, I received for inspection the so-called $\frac{1}{10}$th, with which Mr. Stodder now claims that he has resolved the 19th band. I was, however, unable to push it beyond the 16th. Mr. Stodder had requested me to send it, in case I did not succeed with it, to Mr. W. S. Sullivant, of Columbus, Ohio, a gentleman whose reputation for skill as a microscopical manipulator stands deservedly high in this country. Mr. Sullivant wrote me, July 11, 1869 :—"The Tolles' immersion $\frac{1}{10}$th I returned to Mr. Stodder, who wrote me about

* 'Quarterly Journal of Microscopical Science' for October, 1868, p. 225.

it. Tolles marked the grade of that objective too high. It is hardly an $\frac{1}{8}$th English standard. You put it up to high-water mark in making it resolve the 16th band. I was satisfied in getting it well through the 15th, and took the 16th partly on trust." Nevertheless, after this lens got back to Mr. Stodder, lines were shown with it which it seems a number of my friends in Boston believe were the real ones, though none of them appear to have made a count, which, after all, is the decisive test. I still believe that my friends were deceived by spurious lines, and have greatly regretted that I could not drop in on them, and look over the shoulder of Mr. Tolles (who is regarded not only as the champion maker of objectives, but as the very best manipulator of his own lenses) to see what it was he really saw. Fortunately this has recently been done by a disinterested witness. In Max Schultze's 'Archiv für Mikroskopische Anatomie' (Band VI., S. 205) there has just been published an interesting article by Dr. H. Hagen, "Ueber die Mikroskope Nordamerikas." Dr. Hagen is a German gentleman who has recently settled at Cambridge, and who has been appointed a professor in Harvard College. Since he wrote the paper above quoted, he has paid a visit to the Army Medical Museum, and I have had a long conversation with him about it. I found him well instructed in microscopical matters, and must regard him as a competent witness. He relates on page 216 how Mr. Tolles himself undertook to show him the 19th band with an immersion $\frac{1}{10}$th, which I understand from him to be the very one in question. He showed him lines indeed, but he was unable to count more than forty of them. Mr. Tolles himself counted between forty and fifty. These counts show that the lines in question were spurious. Dr. Hagen concludes, as the result of his examination of the lenses of Tolles, that in a general way they are quite as good as lenses of the same power by the best European makers, but that they cannot be said to excel all others, since as yet none of them have resolved the last four bands of the Nobert's plate, as the $\frac{1}{16}$th immersion of Powell and Lealand has done.* He naïvely relates that his unwillingness to admit more was not favourably received by the Boston microscopists. "Ich kann nicht unterlassen zuzufügen, dass schon mein Versuch, ein Urtheil über die hiesigen Instrumente zu fällen und europäische denselben gleich hoch zu stellen, einen Sturm der Indignation unter den hiesigen Mikroskopikern hervorgerufen hat. Ihre Indignation wird mehr erklarlich, wenn man weiss, dass sie fast sammtlich der Boston optical Association angehören, die bis jetzt ohne Zinsen arbeitet.†

After this testimony of Dr. Hagen I must be pardoned if I continue my disbelief until some evidence more positive than mere opinion is offered.

As to whether I saw the true lines with the $\frac{1}{16}$th of Powell and Lealand, I may simply remark that I supported my statement by *a count of the lines* as well as by the photographs made by Dr. Curtis. I thought, and still think, that Dr. Curtis's photographs were conclusive evidence that I had seen the true lines, although the spurious lines shown on the edges of the band prevented them from serving for

* See page 217 of his paper. † Page 208.

the purpose of a count. I have recently made myself another photograph of the same band which will perhaps serve to convince any who are still incredulous. I send paper prints enlarged from this photograph.* The lens used was the Powell and Lealand immersion $\frac{1}{16}$th.

Very respectfully,

J. J. Woodward,
Assist.-Surgeon and Brevet Lt.-Col. U. S. Army.

PROCEEDINGS OF SOCIETIES.†

Quekett Microscopical Club.‡

At the ordinary meeting of the Club, held at University College, April 22nd, Dr. R. Braithwaite, F.L.S., Vice-President, in the chair, a number of donations to the library and cabinet were announced, nine new members were elected, and five gentlemen were proposed for membership. A paper was read by Mr. N. E. Green, "On Ciliary Action in Infusoria," in which he detailed the results of numerous observations undertaken with a view to the discovery of the cause of the current observed immediately above the contraction of the neck. This was at length satisfactorily ascertained to be produced by a row of minute cilia situated just within the contraction, under what was termed the shoulder. The observations had been carefully verified, and the best means of rendering these cilia visible were minutely described. A cordial vote of thanks to the reader of the paper was unanimously carried, and a short discussion followed, in which Messrs. Curties, Gay, Green, Hainworth, White, and the Chairman took part. The Chairman announced to the meeting the formation of the Croydon Microscopical Society, and expressed a hope that its members might be able occasionally to unite with those of the Club in their fortnightly excursions. The Secretary understood that this would be done for the first time on May 14th, at Carshalton. The proceedings terminated as usual with a conversazione, at which interesting objects were exhibited by Messrs. Curties, Hainworth, Oxley, Quick, and White.

Literary and Philosophical Society of Manchester.

Annual meeting, April 19th, 1870. J. P. Joule, D.C.L., LL.D., F.R.S., &c., President, in the chair.—No papers of microscopical interest were read at this meeting.

* These may be seen [after August] at the R. M. Society's Rooms, King's College.—Ed. M. M. J.

† Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

‡ Report supplied by Mr. R. T. Lew[illegible]

Brighton and Sussex Natural History Society.

May 12th.—The President, Mr. T. H. Hennah, F.R.M.S., in the chair.

Mr. Wonfor reported on the success of the field excursion of the previous Saturday, and announced that Mr. H. Willett had invited the Society to pay him a visit at Findon in June.

The meeting being for the exhibition of specimens, Dr. Badcock exhibited a large piece of fossil wood recently brought from the island of Portland by his brother.

Mr. Penley laid on the table a copy of the first part of 'Flowering Plants of Tunbridge Wells,' by Dr. R. Deakin, in course of publication; and specimens of oak recently picked up at Tunbridge Wells, coloured green by a fungus, *Helotium æruginosum*, growing on it. This affords the green seen in Tunbridge Wells ware. Dr. Hallifax commented on the growth of this fungus, which he had raised from spores obtained from the infected oak. It was easy to trace the *mycelious* threads in their sections.

Mr. Wonfor remarked that this particular fungus growth was found in France and England wherever the Hastings sands cropped out, leading to the supposition that there was something peculiar in the chemical conditions of soil, &c.

The President exhibited a couple of specimens alive of the sea-mouse, *Aphrodita hispida*, dredged up off Brighton a few evenings before, and remarked on the hairs as microscopic objects.

Dr. Hallifax exhibited some very beautiful micro-photographs of his own taking, the most striking of which were,—injected stomach of owl, 40 diam., showing glandular structure; poison-bag of spider, showing poison fluid issuing from it—in putting on the covering glass in mounting some of the fluid was squeezed out; teeth of medicinal leech, 480 diam., in which the true nature of the teeth was shown; sections of proboscis of blow-fly—one transverse, the other vertical—in which the rasping teeth situated on the central disc were very prominent; a curious spine of echinus, tracheæ of silkworm, and tongue of bee.

Mr. Dennant exhibited a bottle of the ooze obtained in the 'Porcupine' expedition, in lat. 47° 35′ N., long. 12° 15′ W., at a depth of 2435 feet; surface temperature, 65° 5′ F.; bottom ditto, 36° 5′ F.; pressure, 457 atmospheres, or nearly three tons to square inch. The ooze was calcareous mud full of Globigerinæ and Foraminifera, and would be shared out among the members at the microscopical meeting.

Mr. Wonfor exhibited cluster cups on the dog-violet (*Viola canina*) and common nettle, an owl cast composed of the fur and bones of mice, eggs and cocoons of the Emperor moth (*Saturnia Carpini*), and forty-three males of the same moth attracted in two days at Polegate and Tilgate by one female: fifty had settled on the box containing her, of which forty-three were secured. He then read a paper on the power possessed by the females of some insects of attracting the males of the same species in large numbers and from long distances. This led to a very animated discussion.

Mr. Hennah announced that Mr. Peake had discovered a *pygidium* in the lace-wing fly (*Chrysopa pera*); this was believed to be an original discovery, and would be exhibited at the microscopical meeting.

Mr. Wonfor announced that the first meeting of the Microscopical Section would be held on Thursday, May 26th, when the President would give an inaugural address. It was hoped that members would bring microscopes and objects, and be prepared with contributions of slides for the Society's cabinet.

Microscopical Section.

May 26th.—The President, Mr. T. H. Hennah, F.R.M.S., in the chair.

This being the first meeting of the Section, Mr. Wonfor, Hon. Sec., stated the reasons for forming and objects sought to be done by the Section, and announced the receipt of eighteen slides from Mr. Hennah, six from Dr. Hallifax, and thirty-eight from himself, for the Society's cabinet, and urged on all the members to contribute.

Mr. Hennah then read a paper "On Systematic *Recent* Examination with Moderate Powers."

As a suggestive subject for consideration at the first meeting of our Section, I have to ask your attention to a few notes on Microscopical Examination, in its application to our objects as members of the Natural History Society. The limited time at our disposal obliges a rather special view; and I propose to take, as my text, "Systematic *Recent* Examination, with moderate powers," believing that, whether we regard the extension of our knowledge of Natural History, the success of this Section, or our own pleasure in microscopic study, it is alike indispensable. Most of the discoveries with the microscope have been made with instruments of moderate power and cost, and have resulted from patient, diligent observation. As we glance through the history of the microscope, we cannot avoid noticing how little has depended upon the instruments, and how much upon the method and perseverance of the men who have accumulated so vast an amount of information; and although the wonderful perfection of modern high powers,—and, indeed, of the microscope generally,—has undoubtedly increased both our means of research and the number of observers, the conclusion is forced upon us that—as we use it—the microscope is almost a new sense, or a mere toy. I hope the growing tendency to the latter result may find no place amongst us. Pride in the possession of a fine instrument, and a consequent desire to exhibit its powers, often leads to the exclusive study of conventional test-objects, which, while it gives command to the microscope in a special way, and stimulates opticians to improvements, too frequently arrests original investigation. The ordinary work of the naturalist can, in the first instance, be better pursued with low than with high powers. Extreme nicety of preparation and elaborate illumination are not required with them, while the excellence of modern halfs and fifths is so great that appeals to higher powers are only occasionally needed. I would not at the same time underrate the importance of having a reserve of power, as some investigat[illegible] be carried on except under the

highest possible amplification. I wish simply to enter a protest against the loss of time involved in the employment of unsuitable means. Whether we use high or low powers we should—in original investigations—be on our guard against the unconscious tendency of the mind to make "the wish father to the thought;" and, although we cannot be altogether free from preconceived ideas, their influence should be limited to the *suggestion of inquiry.*

It is necessary for a just appreciation of our own work (and that of others) that we should be well acquainted with the literature of the microscope. It is, however, already so much scattered that it is difficult to ascertain the actual amount of knowledge on any given subject, and much time is wasted in investigations which should be but past steps in our progress. At the same time, we should not too readily accept authority on matters difficult of proof, as a false idea of the state of microscopical knowledge is frequently given and doubt arises as to our powers of observation or the instruments we are using. I may instance the structure of some of the diatom valves, which, although demonstrated nearly fifteen years ago, was, in consequence of the erroneous views of Griffiths, Wallich, and others, held still to be an open question until, only last year, the President of the Royal Microscopical Society announced *his* demonstration of their structure as a "*New birth to the microscope.*" In making these remarks I wish to acknowledge fully the obligations we are under to men who have laboured so earnestly to our advantage; and in calling attention to exceptional error I am but endeavouring to provoke careful, original investigation amongst us in place of idle reliance on any authority. In the frequent intercourse of men engaged in a common pursuit,—such as it is the intention of this Section to promote,—lies the best substitute for individual experience; the knowledge acquired by any should be available for all, and errors of solitary observation are soon corrected when brought to the test of criticism and comparison. Most conducive to a true knowledge of objects is their examination in a recent state, and an acquaintance with the appearance of ordinary things will be found much more valuable than the settlement of a *diatom* or *podura* question. In the food we eat, the clothes we wear, the parasites that plague us, and the very dust about us, there is a large field for investigation as a necessary preparation for other studies, which has also an interest of its own in a time when the President of the Board of Trade tells us that adulteration is but a form of trade competition. The poor Welsh impostor gave evidence of her fatal deception only a short time since, in the starch which the microscope discovered in her stomach; and many other instances might be adduced to recommend the study of common things. Our principal object, however, should be to inquire into the natural history of our own locality, the minute fauna of which has been but imperfectly examined. Our shore offers every inducement to extend research. The smaller Crustaceans are scarcely known amongst us, although two of the most curious and interesting—the *Caprella* and *Ammothea*—abound on the weed at Kemp Town and give promise of allied species of greater rarity as a reward for search. Acorn barnacles and shore crabs are instances of strange metamorphosis, as are also the polyps, of

which *Laomedea obliqua* and *geniculata* abound. The urn-shaped egg-cases of *Purpura lapillus* and the riband-like strap of eggs of *Doris tuberculata* can generally be found to illustrate the extraordinary embryonic development of the mollusca; there is, in fact, scarcely a limit to the list of subjects waiting for examination. For full appreciation of minute structure comparison with permanent specimens is both necessary and interesting. They cannot, however, be seen under sufficiently varied conditions, and we may as well take an ancient Egyptian as a specimen man as trust exclusively to the mummies in balsam which fill our cabinets; we must, instead—as students of Nature—follow her home and watch her ways patiently, as far as we can. Nothing can be known of the protozoa, or rotatoria, unless we examine them in life. Cyclosis in vegetable cells must in like manner be seen in life to be seen at all. The generation of the cryptogams would be really hidden if the germination of their spores had not been a subject of unwearied attention. The structure of the Foraminifera was not demonstrated by Carpenter without systematic work.

(*To be continued.*)

Bristol Microscopical Society.

April 27th, 1870. Mr. W. J. Fedden, President, in the chair.—The minutes of the two preceding meetings were read and confirmed, and two new members were proposed for election.

Dr. H. E. Fripp then exhibited and described a series of anatomical preparations, chiefly illustrative of diseased conditions of the brain tissues.

May 18th. Mr. W. J. Fedden, President, in the chair.—The minutes of the last meeting were read and confirmed. Two new members were elected, and three gentlemen were proposed as candidates to be balloted for at the next meeting.

Mr. T. H. Yabbicom, C.E., then read a paper "On Raphides."

Dr. H. E. Fripp exhibited and described a peculiar form of dissecting microscope made by Zeiss of Jena. The instrument possesses many peculiar advantages, and was much admired by those present.

Mr. F. R. Martin also exhibited Mr. Browning's new portable microscope.

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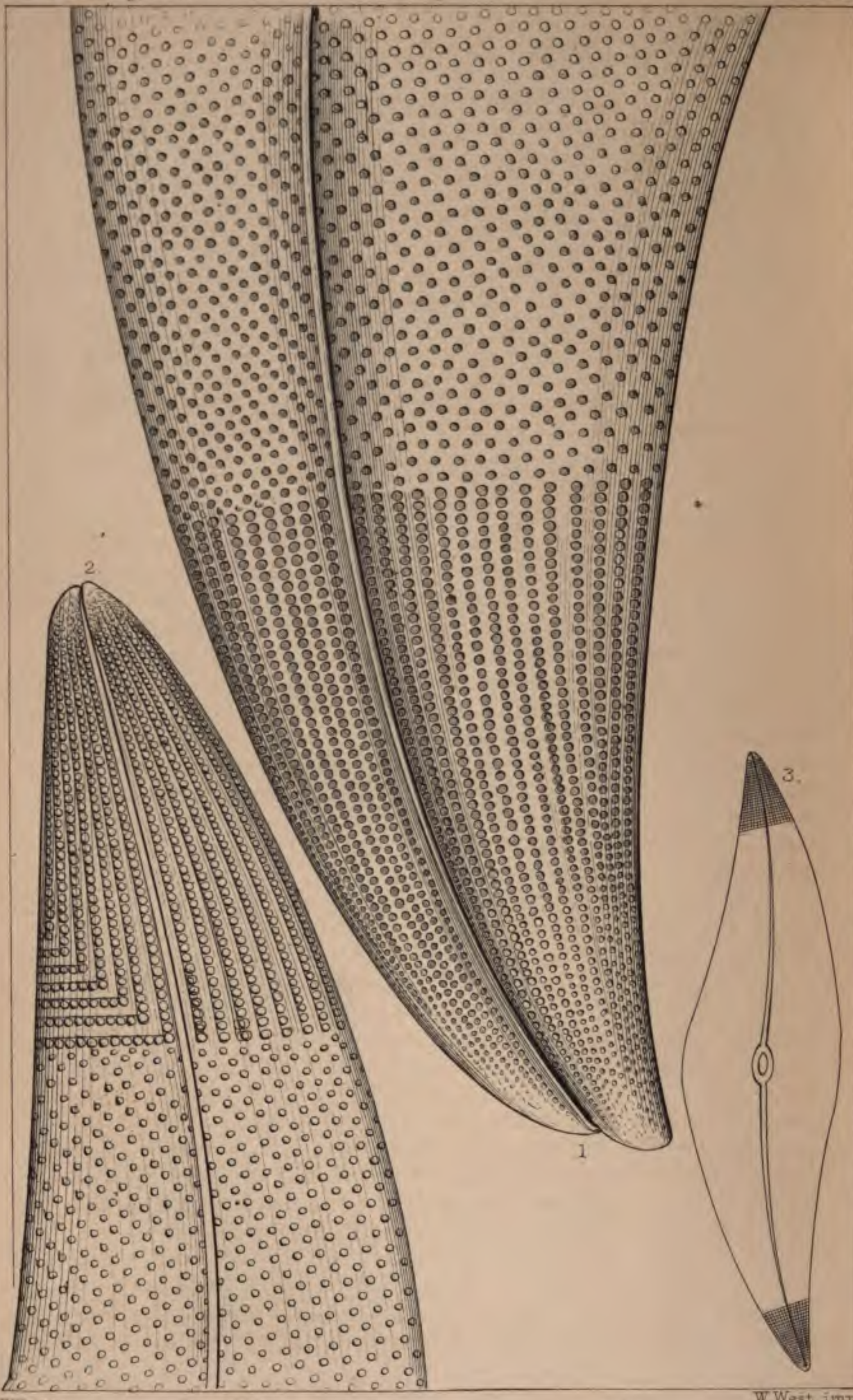
Die zonula ciliaris von Dr. F. Merkel. Leipzig. Engelmann.

Sur les Épines des Echinocidarites, par M. Ch. Des Moulins. Bordeaux.

To Correspondents.—Owing to new arrangements, the Journal goes *to press* earlier than previously. Hence we ha[illegible] unable to insert in the present number communications from Messr[illegible]ack, and Dr. Pigott.



T. West sc. W. West imp.

Peculiarities of Pleurosigma angulatum

THE

MONTHLY MICROSCOPICAL JOURNAL.

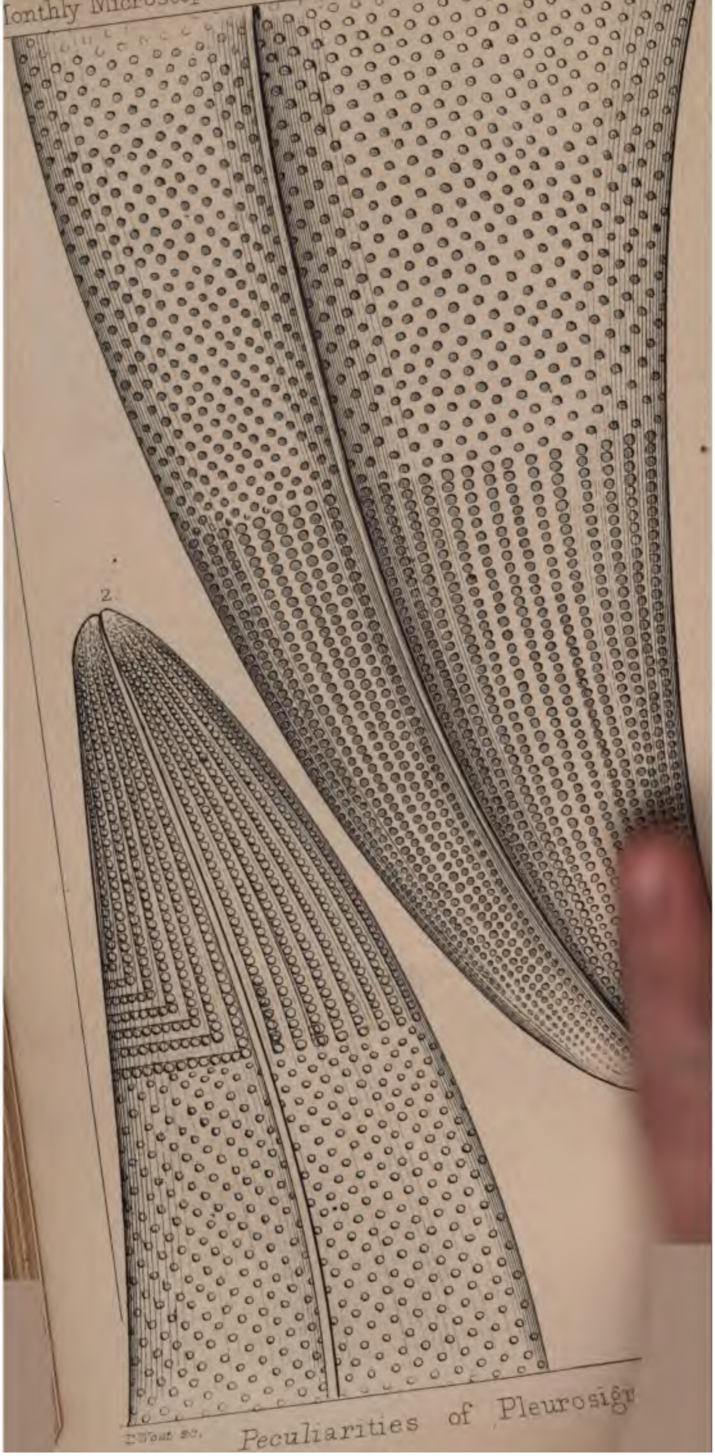
SEPTEMBER 1, 1870.

I.—*On the Structure of the Pleurosigma angulatum and Pleurosigma quadratum.* By John Anthony, M.D., Cantab.

(*Read before the* Royal Microscopical Society, *June* 8, 1870.)

Plate LIX.

I have to call attention to a peculiarity in the structure of the *P. angulatum* and its allied species, which, so far as I know, has never been described. Taking the fact that few microscopic objects have been so frequently and so critically examined as the silicious valves of these beautiful diatoms, not only for the purpose of determining the precise nature of the markings which characterize the Loricæ themselves, but by way of testing the appliances of the microscope, it might fairly be expected that we had fully made out and put on record all that was worthy of note with respect to these shells, but that such is by no means the case I trust to be able to show clearly in the course of this paper. I will begin by saying, that I feel grateful to these minute objects for the lessons they have taught me in the use of the microscope; more particularly in the management of light so as to distinguish between the true and the false; and that I have studied these silicious scales for years, from the far-away time when I was proud of being able to make out crossed lines on their surface with the aid of one of the earliest $\frac{1}{16}$ths constructed, until now, when I am even more pleased to be able to show the nodules, or "dots," as we used to call them, most brilliantly by means of a modern $\frac{1}{8}$th. Having then studied these objects so long and so constantly, I trust it will not be thought that the observations I am about to make are either hasty or given without due consideration. The Pleurosigmata have usually been divided into two sections, *viz.* into those in which the striæ or rows of dots are arranged "obliquely" with respect to the axis of the shell, and into those which are arranged "transversely and longitudinally," or as I should prefer to call it "rectangularly." In the first or oblique division, the *Pleurosigma angulatum* and *P. quadratum* have been unhesitatingly placed, while the second or rectangular division has received *P. hippocampus*, *P. Balticum*, and others well known to



Peculiarities of Pleurosigma

the microscopist. Now if I am right in my observation, this classification of the markings of *P. angulatum* as oblique—that is wholly oblique—is erroneous, as I find the markings, strangely enough, to be of a mixed nature, and it is to this peculiarity I wish to call attention. I have made out most satisfactorily, that in the said *P. angulatum* and *P. quadratum* the ends of the valve, for about one-sixteenth of its length, have not any markings but such as are distinctly rectangular, markings of the character in fact of those seen in the second division, as *P. hippocampus*, *P. Balticum*, &c., the striæ being, as in these last, farther apart transversely than they are in the direction of the axis of the shell. It must not be assumed that these markings are either accidental or few in number, for not only have I found them to be always present, but in what perhaps I may be permitted to call a normal or symmetrical valve, I have counted 13 rows of dots from the mid-rib towards each edge, while I made out the number as 32 to 50 dots arranged in these lines between the obliquely-marked portions of the valve and its end. In the sketches which accompany this paper, I have drawn with the camera lucida an entire Lorica, and indicated exactly the bounds respectively of the oblique and rectangular arrangements. The larger drawing (Fig. 1) represents the appearance of the end of one of these chosen Pleurosigmata under the most favourable conditions of light and definition, and when, as is by no means always the case, the valve is symmetrical. This symmetry is so little constant, that I should venture to say, that the silicious envelope seems subject to what gardeners usually call "sports," that is, that in all the ends having the rectangular markings I have attempted to describe, scarce any two of the specimens would be exactly alike. For instance, the portion of valve on one side of the mid-rib would be rectangularly marked to double the extent of the other, in which case I generally found that the other end of the shell repeated the peculiarity. On a few occasions, I have found the curious "sport" (Fig. 2) which I have indicated in one of the sketches, where the longitudinal close series of dots forming striæ, bend at right angles without a break, and so become transverse; the next series follow the example, till the whole set has somewhat the appearance of a set of carpenter's squares. I think I am not deceived in my observation, that these rectangularly arranged "dots" are really larger in size, or as we are accustomed to call it "coarser," than the usual and well-known "dots" which are arranged obliquely, and they are not quite so easy to resolve, and to deprive of spectra or shadows.

Now I do not want anybody to take this peculiarity of structure in our old friend *P. angulatum* for granted; I trust that each microscopist will examine for himself, it being only fair to say, however, that to make out this structure *well*, will require the very best appliances, and no small amount of care in the use of them. I feel

sure that the structure is there, that it is not an optical deception, and that a fine instrument, a careful manipulation, and a good sight will not fail to make it out. I may say here that I prefer for such an examination a $\frac{1}{5}$th or a $\frac{1}{8}$th to a $\frac{1}{12}$th or a $\frac{1}{16}$th, for the reasons we are now beginning to understand, *viz.* that we can, with the lower objectives, "take cognizance of more planes;" and it must not be forgotten that the ends of the *P. angulatum* and *quadratum* are curved or twisted. Also I may observe that, working as I always do by preference with a rectangular prism instead of a mirror, and consequently with the light directly in front of the microscope, I find that the markings I have indicated are considerably more distinct when the valve under examination is placed vertically or nearly so.

I trust the Society will not consider these details trivial, for independently of making out more perfectly the structure of an object which has always been deservedly a "pet" with microscopists, we are beginning to recognize that we are in a transition state with regard to our knowledge of the true structure of hundreds of objects which were formerly misunderstood, mainly on account of the material in which it was thought necessary to mount them, greater attention having been paid of late to quality rather than to quantity of light for illumination, and the blaze, and the glare, and the "pretty" but false appearances produced by Canada-balsamed objects having been relegated to the class of observers who look on the microscope as a sort of peep-show, instead of a most valuable instrument for scientific research, and towards the improvement of which I am glad to feel so many minds are now bent, each humbly seeking for truth, and none daring to claim even an approach to perfection.

EXPLANATION OF PLATE LIX.

FIG. 1. Extremity of *P. angulatum* (reduced $\frac{3}{4}$ from original drawing).
" 2. Variation of rectangular marking on *P. angulatum* (reduced $\frac{3}{4}$ from original drawing).
" 3. *P. quadratum*, drawn with camera lucida, to show relative extent of oblique and rectangular markings (reduced $\frac{1}{2}$).

II.—*Object-glasses and their Definition.*

By F. H. Wenham, Vice-President R.M.S.

Dr. Maddox has obtained photographs of the *Podura* (*Degeeria domestica*), by means of a Wales' $\frac{1}{8}$th, showing a beaded appearance, similar to that observed by myself, and has kindly offered to leave the negatives with some one who will supply prints to those desirous of possessing them, and who have not succeeded in developing such a structure.

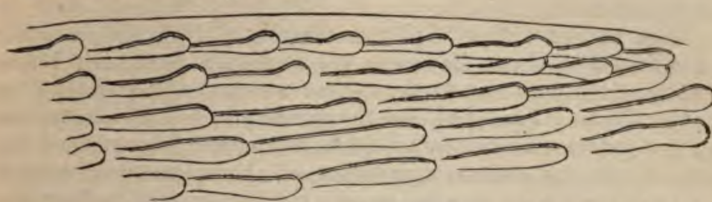
The appearance is obtained at the entire sacrifice of the "note of admiration" markings, and that distinctness and sharpness of definition that so delights the eye of the optician, and resembles an irregular ribbing, as if a painting had been made on a panel and the graining comb passed transversely in a slightly undulating course across the ribs, and it is remarkable that where the cross lining has commenced with a larger nodule, this increased size has a tendency to continue right across the scale.

I have recently made some careful examinations of different species of *Podura*, and cannot confirm Dr. Pigott's statement that the "note of admiration" markings are caused by the crossings of a series of oblique ribs on the upper and under sides of the scale. I find that the best illumination is sunlight: without sufficient reason this is generally repudiated. In cases where the colour correction of the object-glass is imperfect, the chromatic errors are so developed, that accurate observation is difficult; but with a glass correctly achromatic, sunlight is really a valuable aid to investigation.

I have never seen the varieties of *Podura* so finely shown as by this means. I use no stop under the stage, but throw sunlight on the scale in a direction from the top, towards the root, at an angle of about 30° with the *concave* mirror. The characteristic markings of the scales are thus splendidly shown by the $\frac{1}{25}$th of a blue colour on a pale-green ground. The highest eye-piece may be used, and the tube drawn out to its utmost length, without sacrifice to distinct vision, thus obtaining a magnifying power of near 11,000 diameters. "The note of admiration" markings are only seen *in focus*, and then show scarcely the vestige of a beaded structure. By focussing away the markings fuse together lengthwise, and appear like irregular knotted cords—the so-termed "beads." The outline sketch (p. 125) represents an oblique view taken by the camera lucida of the markings on *Podura*, magnified about 6000 diameters. The margin of the scale (mounted on the covering glass) had curled away so as to bring several of the ribs gradually into profile.

Up to the present time I have been unable to find any of the proper insects in my locality in order to fulfil my intention of torrefying the scales as they lay on the glass, for the purpose of partly

destroying their transparency and rendering them brittle enough to be broken into fragments.



The evidence, as it now stands, that the markings are elevations of the above figure, is as follows:—With the parabolic condenser (which approximates to an opaque illumination) the markings have the same characteristic form, and appear as material bodies of denser structure. Some years ago Mr. Ross showed me a *Podura* scale, mounted, uncovered, on a black disc, illuminated strictly as an opaque object, by a Leiberkuhn, which he had adapted to a $\frac{1}{8}$th of large aperture. The "note of admiration" markings were finely displayed, and appeared like detached bodies due to surface elevations. This structure was afterwards confirmed by myself by spreading the scales on a highly-polished piece of Daguerreotype plate, and throwing a silver electro-deposit upon them. The detached metal film was then washed with a solution of caustic potash, in order to get rid of the scales, the matrices of which plainly showed the same surface form of markings. The late Richard Beck investigated this test as an opaque *uncovered* object with a $\frac{1}{8}$th. The light was thrown on the scale by condensing lenses, and he gave illustrations of the appearances in his usual graphic style and published the following remarks:—"When the markings are at right angles to the direction of the light, they are illuminated on the sides farthest off. When they are in the same direction as the light, with the narrow ends pointing to it, the broad ends appear like brilliant spots; but when the direction is reversed, the light from the points is so slight that the scales appear to have lost their markings altogether. Now, if the object were an opaque substance this result would have been a convincing proof that the markings were depressions; but as we know it to be transparent, it follows that these particular appearances can only be produced by elevations."

In July last year I communicated to this Journal a principle of illumination by laying the objects on a glass surface, from which light was totally reflected, and using under such conditions the ordinary slides upon which objects are mounted. At all points of contact light will be admitted through, giving an analysis of the form of such parts. In the *Podura*, as the light is thrown on more obliquely at an angle where total reflexion again begins to prevail, the scale appears marked over its surface with bright blue dots,

which arise from the thick end of the "note of admiration" markings being the last to transmit the light, showing that these are real prominences.

With respect to the "Aplanatic Searcher," it would be unfair to Dr. Pigott to discuss its merits, till we have a description before us of its construction and use, so that we can adapt and try it in the same form. I am glad that the optical principle and correction of the objective-glass is now brought prominently forward, and the effects of spherical aberration investigated. I may, however, remark that in the adjusting glasses, as sent out by our best makers, the effects of *positive* spherical aberration only can be observed, as some range of this is given by the closing of the lenses for the purpose of balancing the negative error produced by glass of different thicknesses. In order to observe the effect of negative spherical aberration on a globule, the index tongue must be taken off the mount, so that the glasses can be separated beyond the range to which they were limited by the maker. This is the neutral point between the two errors, where all aberrant rays are brought to one exact plane, the APLANATIC FOCUS of Mr. Lister.

III.—*The Ciliary Muscle and Crystalline Lens in Man.*

By J. W. Hulke, F.R.C.S., F.R.S.*

In the course of lectures "On the Minute Anatomy of the Eye" which I had the honour of delivering before the College of Surgeons last summer, and which were published in this Journal, I described the histology of the cornea and vitreous humour, of the retina and tunica uvea.

There remain the parts concerned in the accommodation of the eye, and the conjunctiva, sclerotic and optic nerve, an account of which will complete the normal anatomy of the eye-ball.

I propose to devote the present lecture to the apparatus of accommodation.

Let me in the first place endeavour to explain what the term *accommodation* technically means.

That we cannot see perfectly distinctly at the same instant two objects placed at different distances before the eye is a fact of the truth of which a moment's attention suffices to convince the most unobservant person. The fact is most easily realized when the objects are near, for when they lie at a great distance from the spectator the minor distinctness of one of them is less appreciable, but when they are relatively close to the spectator it is impossible

* Being the first of the Aris and Gale Lectures delivered during the present year at the Royal College of Surgeons.

for him not to become aware of the phenomenon. Thus when I look at the nearer of two trees placed several yards apart nearly in the same line, in a distant field, the minor distinctness of the farther tree is so slight that I may fail to notice it; but when I look at a book through a veil, both being near me and only a few inches apart, I find that when my eye is fixed on the print I see it quite distinctly, while I am scarcely conscious of the presence of the intervening veil; and again, when I look intently at the veil and perceive its texture distinctly, at that same moment the print becomes confused and unrecognizable. What is the explanation of this?

In order to see an object distinctly, an exact image of it must be formed on the bacillary layer of the spectator's retina. Every luminous point in the surface of the object turned towards the spectator must be represented by a corresponding image-point upon his retina. This is effected by the refractive power of the eye—that power its transparent parts possess in common with inanimate transparent bodies, of changing the original direction of the luminous rays which enter it, and of giving these rays a new direction of a kind dependent on their relative densities and their curves.

Luminous pencils coming from remote objects consist of parallel rays, and, having regard to the small opening of the pupil, the pencils which enter the eye from an object 20 or more feet distant may be considered to be composed of parallel rays. Now the refractive power of the eye is such that parallel rays entering it are collected in exact foci upon its retina without the exercise of any vital effort, the eye itself being quite passive. It would occur as well in a dead eye so long as its media remained transparent and while they retained their proper curves.

The luminous pencils which a near object sends to a spectator's eye consist of divergent rays, and the unaided refractive power of the eye which sufficed to unite the parallel rays from a distant object in the bacillary plane of the retina, is insufficient to collect divergent rays in exact foci in this plane. The foci of these rays lie behind the retina which the pencils strike as spots, the sections of cones, called circles of dispersion, and not as points. The result of this is a blurred confused image, and not a clear one, the production of which requires the rays to be brought to exact foci in the bacillary layer.

We are, however, conscious that we possess the power of seeing distinctly near objects as well as distant ones, which proves that the eye has the power to unite divergent as well as parallel rays in exact retinal foci; and this implies the possession of a power of altering its refractive state so as to suit it to the distance of the object we desire to see distinctly, or, in other words, to adapt it to the degree of divergence of the luminous rays entering the eye from the object.

This adaptation of the refractive state of the eye is technically called its *accommodation*. It has been at different times attributed to a change of the figure of the eye-ball, to an alteration of the curve of the cornea, and to a shifting of the position of the lens; but more delicate methods of observation than were formerly at the command of the physiologist have shown all these views to be erroneous, and by the direct inspection of the human eye with instruments specially contrived for this investigation, it has been demonstrated that its accommodation for a nearer object is effected by increased convexity of the lens, and this chiefly of its anterior surface, the curve of its posterior surface being altered in a scarcely appreciable degree. With the increased convexity of the lens, its axis is proportionately lengthened, the pupillary region of the iris approaches the cornea, and the peripheral portion of the iris recedes from it.

There are not any grounds for supposing that this change of figure is wrought by any power inherent in the lens itself, which is plastic, but is not endowed with contractile irritability, and is not dominated by the will being devoid of nerves.

The active factor of accommodation must, therefore, be external to the lens. Now in close relation to the lens there are two muscular organs—the iris and the ciliary muscle. The existence of accommodation in persons from whom the iris is congenitally absent, and its persistence where the iris has been in part or entirely removed, demonstrate its independence in man of this diaphragm. There remains, therefore, only the ciliary muscle as the active agent of accommodation in the human eye. To the ciliary muscle and lens I would invite your attention to-day. I shall take the lens first.

Lens.—The lens of the human adult has a flattened bi-convex figure. The anterior surface is less convex than the posterior, the radius of curvature of the former being nearly twice as great as the radius of the latter surface. The infant's lens is more nearly spherical, which makes the distance between its summit and the cornea smaller than in the adult's eye. This circumstance is not without influence in the causation of the minute white speck on the front of the lens (the central sub-capsular cataract) not unfrequent in persons who have suffered from infantile purulent ophthalmia, even where this has not been complicated with perforation of the cornea. The compression of the cornea by the swollen eye-lids and œdematous conjunctiva, and a slight amount of deep congestion pushing forward the lens, may bring the lens and cornea together, and thus disturb the nutrition of the growing lens at the point of contact, and induce a perverted growth and retrogressive changes in the lens-tissue.

In other mammalia, and in the lower vertebrata, the figure of

the lens is less flattened than in adult man, resembling more nearly the shape of the human fœtal lens.

The lens of vertebrate animals is formed almost entirely of a peculiar fibrous tissue, with a very scanty formless interstitial substance. It is enclosed in a shut capsule, the integrity of which is of the highest importance to its transparence. This capsule is a perfectly transparent, very elastic yet brittle, membrane. It is little prone to degenerative metamorphosis and never undergoes absorption. I have found it transparent and unchanged sixteen years after the extraction of the lens for cataract. Its chemical constitution and reactions resemble those of the other hyaloid membranes, it is unaffected by weak acids and alkalies, and it resists putrefaction. With high magnifying powers no indications of structure are discernible in it, except faint marks of lamination in the stout capsules of the large mammalia. The front half of the capsule, or more exactly that part of it which lies in front of the attachment of the suspensory ligament, is thicker than the posterior half; at a rough estimate its thickness may be taken to be three times as great.

This difference and the unavoidable implication of the front half of the capsule in operations for the removal of cataract, have led to the adoption of the terms anterior and posterior capsule. As these terms are convenient, their use is not objectionable if it be borne in mind that they refer to two halves of one and the same sac, and not to two distinct sacs.

Besides being stouter, the front half of the capsule differs also from the posterior, in being lined with an epithelium. This consists in the central region of a single layer of large, flat, polyhedral cells, each enclosing a circular nucleus. These nuclei are remarkably uniform in size and shape. At the edge of the lens the epithelial cells are much smaller, and so closely crowded that their nuclei are separated by very small interspaces. In mature lenses the marginal epithelium is composed of only a single layer of cells, but in young and growing lenses it is formed of several layers of cells, with an imbricated arrangement which constitute the matrix, out of which the fibrous tissue of the lens is evolved.

The capsular epithelium plays an important rôle in the so-called capsular opacities accompanying various forms of cataract, congenital as well as acquired, for out of it are evolved, by what may be called a perverted development, nucleated fibrous webs, often of great toughness and density, underlying the inner surface of the capsule. It must not be forgotten that the capsule itself never becomes opaque, what are called opacities of the capsules being always deposits of opaque substances and adventitious growths upon its surfaces. These may exceptionally be overlaid by a transparent colloid substance, and then an opacity may seem to be seated in the

capsule, but the colloid mass is not a part of the capsule, it is something superadded to it.

I am aware that some good observers deny this origin of these intracapsular fibrous webs, which are not optically distinguishable from a connective substance. They ask how can a connective substance be the derivative of an epithelium, and they maintain that in all these cases the capsule has not been entire, but it has had some rent through which these fibrous webs have intruded themselves from without. I cannot yield to this opinion, because I have found such webs on the inner surface of capsules, which I could not doubt were entire and where their intrusion was impossible, and because I have, as I believe, been able to trace the evolution of the fibrous tissue from the epithelial cells through intermediate phases. A normal lens-fibre, which all allow is the final phase of a capsular epithelial cell, differs hardly less from its initial phase than do some of the elements of these fibro-nucleated webs.

The posterior half of the capsule has not any epithelial lining, but its inner surface frequently exhibits polyhedral marks which have been mistaken for an epithelium. They really express the hexahedral, oblique cross-sections of the swollen posterior ends of the lens fibres.

The tissue composing the lens itself consists in greatest part of long, flat ribbon-like fibres. These have two wide surfaces and four narrow ones meeting in two thin bevelled edges, which give to their cross-sections a hexahedral figure. The fibres are really long tubes filled with the protein substance, to which chemists have given the name Globulin. When the fibres are broken across, it transudes in the form of globules from their ends. It often accumulates in large masses between the layers of fibre in lenses, which have been hardened with chromic acid.

A nucleus is present in the superficial fibres near the edge of the lens, but the deeper fibres are more sparingly nucleated. The fibres cohere very closely by their flat surfaces, and still more intimately by their bevelled edges. These latter in some vertebrates are serrated, which renders their union still more secure. This serration is very coarse in fish and in some chelonia, much finer in snakes, so fine in frogs that the edges appear only as if slightly frayed, and absent from the human lens-fibres.

The other constituent of the lens is the interstitial tissue, a formless substance present in a very minute quantity in the axis of the lens, and in those extensions of it called the central or axial planes. In young persons the refractive index of this substance agrees with that of the fibrous tissue; hence their lenses are free from the *internal reflexions* which the lenses of elderly persons exhibit, and which give these an *opalescence* which an incautious observer may readily mistake for a cataractous opacity.

The axis in the simplest form of lens as that of some fish and of amphibia is a streak or line of this interstitial substance traversing the centre of the lens. The lens fibres are grouped around it in the manner of the meridian lines upon a globe, the inner fibres progressively shortening towards the centre of the lens. In other fish, and in the porpoise and the rabbit amongst mammals, the axial streak is flattened. It extends in two opposite directions and forms a central plane, the ends of which make a linear stigma on the front and back of the lens. The directions of these stigmata do not coincide, they intersect at a right angle as they would if the plane of which they are the ends had been twisted through 90° in passing through the lens. From the two edges of this which may be distinguished as the primary plane, others, secondary planes, run out in a complicated manner towards the circumference of the lens. The primary central planes in most mammalia diverge at equal distances of 120° from the axis, and their ends form on the back and on the front of the lens a trifid stigma. The rays of one stigma intersect the angles included between those of the other stigma, as they would do if the tripartite axis had been twisted through 60°.

In these lenses the arrangement of the fibres with respect to the central planes is much more complicated than in the simple amphibian lens. All its details are probably not yet known to us, but so far as they have been ascertained, it appears that the fibres pass between the front and back of the lens, winding round its equatorial edge in such a manner that a fibre starting from the interval between two of the front planes falls behind on the edge of a posterior plane, while a fibre starting from the edge of one of the front planes would fall behind in the angle made by two of the posterior planes. The intervening fibres between these extremes take intermediate positions on the planes.

The tripartite division of the axis persistent in many mammals, is present also in the human fœtal lens, which has been already mentioned is nearly spherical. As the lens enlarges, the three primary planes detach secondary and tertiary ones, the multiplication continues during the whole period of growth, until in the human adult the minor planes form an excessively complex frame.

With this excessive complexity of the planes the fibres maintain a general direction between the front and the back of the lens, and since the surfaces of the fibres cohere less strongly than their edges, most lenses when artificially hardened can be split by a coarse dissection into concentric lamina.

In youth the lens is soft, but with advancing years it acquires greater consistence, becoming in aged persons really hard. It is to this change, in consequence of which the lens becomes less plastic as we advance in life, that presbyopia is mainly due. In birds and

in lizards the nucleus only is concentrically laminated, and the outer fibres pass vertically and obliquely between the capsule and nucleus.

Connections and Relations of the Lens.—The back of the lens lies in a hollow in the front of the vitreous humour, which is bounded by an extension of the hyaloid capsule of this humour, which is so distinct from the posterior lens capsule that I should hardly have mentioned their separateness had this not been recently denied. It bends inwards near the edge of the lens, and forms the posterior wall of the space known as Petit's Canal. The lens may be removed in its entire capsule, without destroying the integrity of this partition between the vitreous humour and the lens-bed. But its chief support is the suspensory ligament which slings the lens to the ciliary processes. This arises from the whole inner surface of the ciliary body in front of the ora retinæ, from columnar epithelial-like bodies resting in the pigmented epithelium, and it is attached in a plaited manner to the capsule at the edge of the lens, advancing slightly upon the front, and to a less extent on the back of the lens. It is made of fibres, which chemically resemble yellow elastic tissue. They are remarkable for their hard, sharp outlines, and their clean fracture. They break up near the lens into brushes of very fine fibrillæ, the interspaces between which are occupied with delicate membraniform expansions. Kölliker regards the elongated bodies from which these fibres arise as special modifications of the connective tissue radial fibres of the retina, which are continued in front of the ora, the nervous retinal elements ceasing at this boundary line. The place of the attachment of the suspensory ligament to the lens varies within rather large limits in different animals; in birds and reptiles it is nearer the front of the lens than in man.

Let us now turn to the muscular apparatus of accommodation. In the human eye the ciliary muscle is the active factor of accommodation. When the cornea and sclerotic are removed a greyish ring is seen behind the iris, on the outer surface of the ciliary body. It was considered ligamentous until Professor Brücke and Bowman nearly simultaneously discovered its muscularity.

In mammalia the muscular tissue is unstriped. The deepest bundles of muscular fibre, those which are in close relation to the outer surfaces of the ciliary processes, are very obliquely directed; collectively, they form a ring resembling a sphincter, yet not completely separable from the other muscular bundles. Attention was first drawn to these circular fibres by the late H. Müller, who conceived that acting through the intervening ciliary processes they might compress the edge of the lens.

The outermost bundles of muscular fibre run in meridional lines. They stream backwards from the cornea and lose themselves on the

outer surface of the ciliary body and choroid. These fibres are connected in front with the middle of the three divisions of the posterior elastic lamina of the cornea (the inner division of this lamina spans the margin of the anterior chamber and forms the pillars of the iris, and the outer division passes backwards and outwards to the sclerotic behind the space known as the circulus venosus, or Schlemm's Canal).

The shortening of these meridional bundles of muscular fibre will tend to approximate their corneal and choroidal attachments; and if we regard the corneal one as the more fixed point, a view which best harmonizes with the anatomical facts, the contraction of these bundles tends to draw the choroid forward and tighten it upon its contents. According to this view the ciliary muscle (as regards its radial bundles) is a tensor of the choroid, as Brücke named it.

Let us next see what light these anatomical data throw on the process of accommodation. You will recollect that in accommodation for a nearer object the lens, as a whole, does not shift its place, but its anterior surface becomes notably more convex, and the convexity of its posterior surface is very slightly increased. With this alteration of its figure the axis is lengthened and the transverse diameter shortened. The pupillary region of the iris approaches the cornea, and the circumference of the iris retreats from it. The lens, with its capsule, is elastic, but without contractile irritability; its rôle is passive. When the suspensory ligament is tight, it must exert traction on both surfaces of the lens (chiefly on the front, by reason of the greater stoutness of the fibres, and of their attachment to the lens advancing rather farther from the edge of the lens), tending to compress the lens in the direction of its axis, and to flatten it—the shape of the lens, in looking at a distant object, which is a passive act, not requiring accommodation.

When the radial or longitudinal bundles of the ciliary muscle contract, and the distance between their extreme points of attachment is lessened, the previously tense suspensory ligament is relaxed, and the lens, no longer compressed by it, becomes more convex by virtue of its own elasticity. If at the same time the circular bundles of the muscle were to shorten, this would tend to contract the circle of the ciliary processes, by which the suspensory ligament would be still more slackened; I doubt, however, whether they can act as a compressor of the edge of the lens.

The ciliary muscle derives its nerves from the lenticular ganglion. They pierce the sclerotic, not far from the optic nerve, and gaining the inner surface of this coat, they run forwards between it and the choroid till they reach the ciliary muscle, on the outer surface of which they break up and re-combine in the well-known

beautiful plexus, a large portion of which is, however, destined for the innervation of the iris. From this course plexus bundles of nerve fibres dip into the muscle, in which they form a finer net, from which single fibres of extreme tenuity are traceable for long distances amongst the muscular bundles, but I have not yet discovered the actual nature of their ultimate connection with the muscular fibre. In my last course of lectures I adverted to the occurrence of ganglion cells in this plexus. They first became known to me by the beautiful preparations of Schweigger, and are not the coarser gangliform swellings recognizable under slight enlargement, described by Dr. R. Lee, jun. The arteries of the ciliary muscle are drawn from the circulus arteriosus major iridis, which distributes many recurrent twigs to it. There are not unfrequently offsets of the arterioles, which this arterial circle sends to the ciliary processes. The venous blood escapes in two directions, posteriorly through veinlets, which join those of the ciliary processes, and lead to the venæ vorticosæ, and in front through veinlets which empty their contents into the circulus venosus in Schlemm's Canal.

IV.—*On the Optical Advantages of Immersion Lenses, and the Use of Deviation Tables for Optical Research.* By Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., formerly Fellow of St. Peter's College, Cambridge.

Part II.

Plate LX.

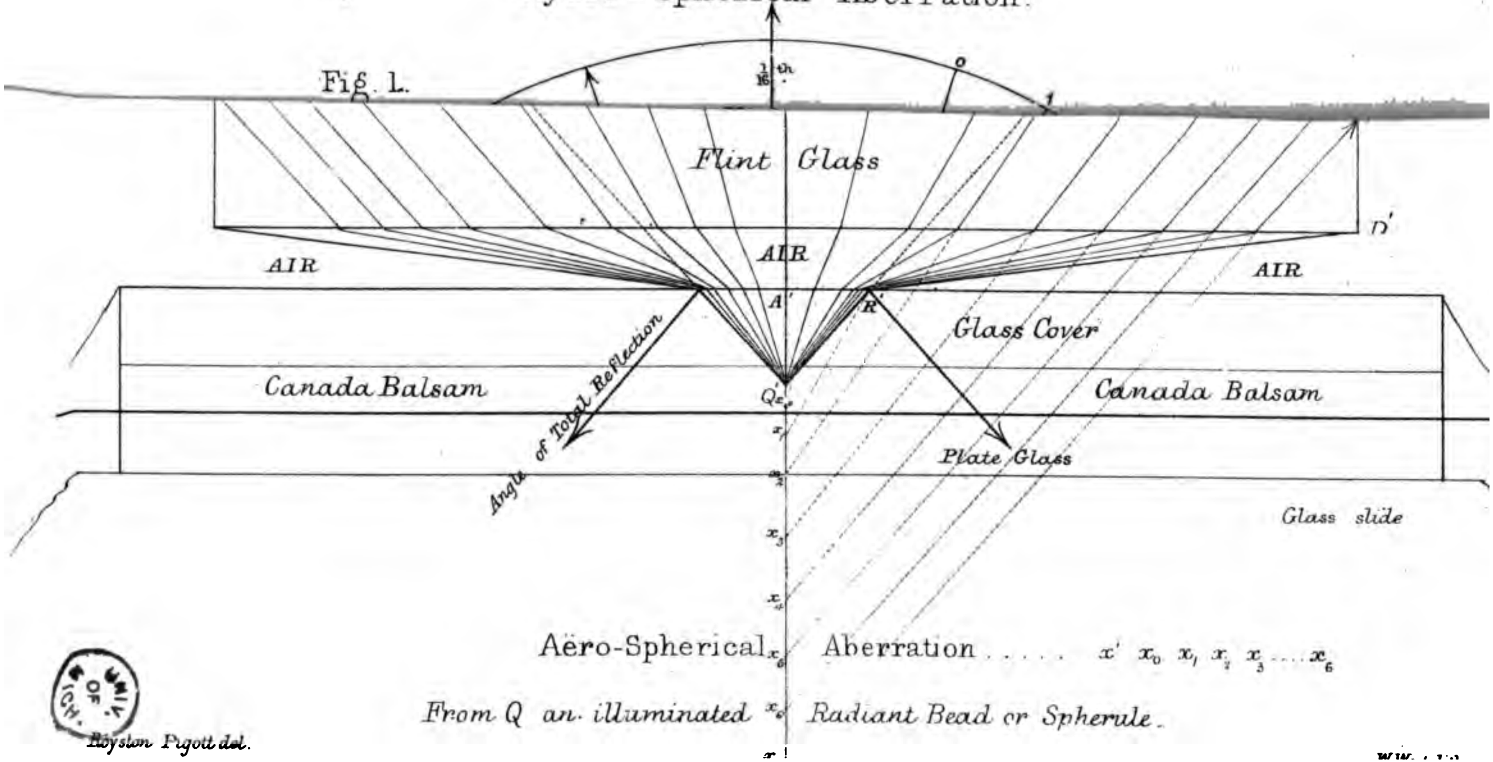
In the first part of this paper I had the honour of drawing the attention of the readers of this Journal to the relative quantity of rays composing a nascent pencil emanating from an illuminated particle immersed in Canada balsam, in the two cases either when a film of water or when air intervenes between the covering glass and the facet lens of the objective; and I ventured to offer a comparative Table of Deviations, which rays of light entering the front surface of the objective had already suffered according to the media of transmission: up to 50° the deviation appeared three times less *viâ* water than *viâ* air. The angle of total internal reflexion in the two cases being—

For plate-glass and air, 41° 48′
" " " water, 62° 57′

When objects are mounted in the dry way instead of Canada balsam, every deviation is changed both for dry and immersion

Compensating Action
or
Hydro-Spherical Aberration.

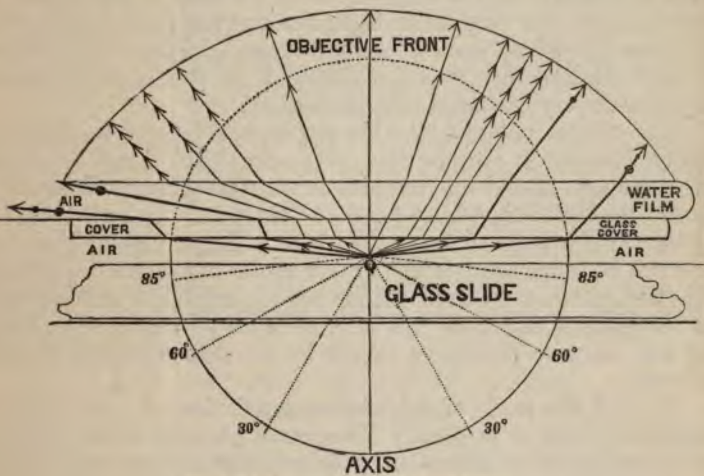
Fig. 1.



Royston Pigott del.

lenses. The nascent pencil successively encounters variable resistances, and finally, in the water lens, attains much less obliquity of transmission.

And, as explained in the first part, an immersion objective of 80° aperture gathers as large a nascent pencil from a shining par-



Object Q being mounted dry without Balsam

ticle Q as a dry objective of 120° aperture: the great bulk of the nascent rays passing more centrically through the water film.*

I will avail myself here of some known results of the immersion system.

Dr. Woodward could only effect the resolution and enumeration of Nobert's XIXth Band by means of the Powell and Lealand Immersion $\frac{1}{16}$th, not by the dry lens. These opticians cannot show the markings of the *Acus* or *Amphipleura pellucida* except by the immersion lens, which, as already stated, is able to gather up a volume of oblique rays nascent from the illuminated particle *viâ water*, which are impossible *viâ air* (for a Canada-balsam mounted object); the quantity of nascent rays, and which are really the best defining rays, being, as compared with air refraction, as $2\frac{1}{4}$ to 1, to be shown farther on.

Again, if the minute spherular bodies detached from diatoms —especially those scattered in the vicinity of Angulata—be steadily examined both with the dry and immersion lens of the finest quality, the confused halo disguising the true definition of the particle as a luminous fog is much less apparent in the water lens. The

* The course of each of the rays is laid down by the Deviation Tables.

beading is blacker. A broken line of beads is given with unusual sharpness as black as jet. The light playing in the focus of the minute refracting bead or lens, conforms more closely to the phenomena of a brilliant point—it swells and contracts with a change of focus—and its colours are transformed. We cannot yet see the black ring which undoubtedly surrounds the bead *in nature*, when viewed singly, but crescentric black shadows are formed *en masse;* the halos of each spherule becoming commingled together, and the jet black shadows then start forth through the general haze into an exquisite tracery of shaded silicious beading.

The results obtained in this paper would seem to strongly justify Hartnack's observations. He asks, "Are large apertures an advantage to the microscopist or to the optician?" and declares "the disadvantages are for the latter only." It is clear that if by the aid of a water or other fluid film, a larger nascent pencil of rays can be transmitted to the eye by means of a smaller apertured objective, whose corrections are far less expensive and difficult than for the glass of enormous aperture, then the advantage of destroying the excessive deviations caused by the aëro-refraction is self-evident.*

To find the angle of total internal reflexion of a nascent ray originating from a brilliantly illuminated particle under different conditions of optical refraction, from a denser medium into a rarer, as from glass into air, water, or oil of turpentine, it is only necessary to take the largest angle, which causes the emergent ray to verge upon an angle of 90°, whose sine is unity. If, therefore, this angle be called the limiting angle of refraction, since

$$\text{Sin. } \phi = \mu \text{ sin. } \phi'$$

In the limit,

$$\text{Sin. } \phi' = \frac{1}{\mu} \text{ sin. } \phi, \text{ when } \phi = 90^\circ$$

$$= \frac{1}{\mu}.$$

Giving therefore μ the values ascertained for refractions between the given substances, ϕ' the limiting angle of refraction at which total internal reflexion takes place is readily ascertained, as shown in the Appendix by Logarithmic Tables.

* Nobert's lines are cut upon the under surface of the covering glass, and therefore the defining rays proceeding from a point in the groove may be considered as *immersed*, or rather formed in the glass so as to produce the same effect nearly as a body mounted in balsam. On this account water lenses, or lenses immersed in other fluid (especially such as have a refractive index nearly equal to that of the covering glass), transmit extremely oblique nascent rays throughout more centrically than the dry.

TABLE II.

LIMITING ANGLES OF REFRACTION OR ANGLES OF TOTAL INTERNAL REFLEXION.

MUTUAL REFRACTIONS.	Angle of Total Internal Reflexion.
	° ′
From plate glass into air	41 48
" " water	62 57
" flint glass into crown glass	56 50
" " Canada balsam	78 23
" " turpentine	78 37

These angles are the extreme semi-apertures of the transmitted pencils nascent from the brilliant particle; all rays of greater obliquity being internally reflected.

In the case of water *versus* air the quantity of defining rays nascent from the brilliant particle immersed in balsam varies as the square of the angles of total internal reflexion in each case; or, denoting these quantities by Q and Q′ for water and air respectively, and the angles by θ, θ',

$$Q' : Q :: (\theta')^2 : (\theta^2) :: \overline{62^\circ{\cdot}7}|^2 : \overline{41^\circ{\cdot}8}|^2$$
$$:: 2\tfrac{1}{4} : 1 \text{ very nearly.}$$

In other words, the water film possesses the advantage of transmitting a quantity of defining rays *nascent from the brilliant spherule immersed in balsam two and one-fourth greater than that transmitted by the air film to the eye through the objective, or in the proportion of* 9 *to* 4. In other words, the definition accomplished *viâ* water exceeds that *viâ* air by more than a double brilliance of effective rays.

As these limiting angles of refraction represent semi-apertures, by doubling them it will be seen that no nascent pencil can reach the objective *viâ air* and *viâ water* respectively greater than

	°	° ′
For air	84	(2 × 41 48 nearly)
" water	126	(2 × 62 57 ")

showing the advantage of the employment of a water film compared with air.

The battle of the glasses is by no means yet fought out. The combinations for their construction have by no means been exhausted.

The deviation caused by using Canada balsam in contact with the covering glass is slight.

The amount is readily found by subtracting the angle of incidence from that of refraction. In all these questions, it saves a great

deal of time to remember that when a ray passes from a rarer into a denser medium, it is bent *towards* the perpendicular or normal at the point of emergence, and *from it* when the ray passes from a denser medium into a rarer.

Thus, vision under water embraces nearly the whole horizon; and the image within a hollow sphere, impacted in glass plate, embraces nearly 180° of aperture, crowding the whole prospect into an exquisite miniature. No eye embraces so wide a prospect as that of a submerged animal.

To find the deviation between plate glass ($\mu = 1 \cdot 500$) and Canada balsam ($\mu = 1 \cdot 532$). Suppose a ray passes from the latter into the former, then

$$\text{Sin. } \phi' = \frac{1 \cdot 500}{1 \cdot 532} \text{ sin. } \phi = \cdot 9791 \text{ sin. } \phi.$$

If $\phi = 15°$, ϕ' will be found $= 14° \; 40'$,* and subtracting ϕ' from it, deviation is only 20′.

The deviation up to nearly 45° is just about 2 per cent. for mean rays.

At 60° it becomes 2°; at 70° nearly 3°; and at 80° nearly 4°; and at 85°, for the extreme aperture of the best objectives, 170°, ϕ amounts to

	°	′	
	85	0	(incidence)
	77	40	(refraction)
Deviation or difference =	7	20	

or the Canada balsam only causes as little deviation at 85° as water at 50° and air at 20° of incidence.

In tracing the rays it will be easily remembered that *whenever amongst several parallel plates the ray is examined, its direction in two identically refractive media is always found to be the same after any number of intermediate refractions.*

The deviation between crown glass and flint glass is found in the same manner; the index of refraction for the passage of a ray between them being $= \dfrac{1 \cdot 642}{1 \cdot 500} = 1 \cdot 09466$, as explained in Part I.

Giving therefore a variety of angles to ϕ from 1° up to 90°, the following Tables have been calculated.† The method by which this is generally done is given in the Appendix.

* See Appendix.

† The tables employed ranged to six places of decimals for the sines of ϕ. If five plans only be used, rather different results will be obtained.

TABLE III.

DEVIATION.

Refraction between Heavy Flint Glass and Plate Glass.*		Obliquity ϕ of Ray passing through rarer medium.	Refraction between Flint Glass and Canada Balsam.	
ϕ'	Deviation.		Deviation.	ϕ'
° ′	° ′	°	° ′	° ′
0 50	0 10	1	0 1	0 59
1 40	0 20	2	0 2	1 58
2 30	0 30	3	0 3	2 57
3 20	0 40	4	0 5	3 55
4 10	0 50	5	0 6	4 54
8 21	1 39	10	0 13	9 47
12 31	2 29	15	0 19	14 41
16 38	3 22	20	0 26	19 34
20 43	4 17	25	0 33	24 27
24 44	5 16	30	0 41	29 19
32 33	7 27	40	0 59	39 1·5
39 53	10 7	50	1 23	48 37
46 28	13 32	60	1 58·5	58 1
51 52	18 8	70	3 1	66 59
55 32	24 28	80	5 17	74 43
56 30	28 30	85	7 38	77 22
56 50	33 10	90	11 37	78 23
Angle of Refraction.	Deviation.		Deviation.	Angle of Refraction.

Another substance may be used for immersion lenses—*turpentine*—whose refractive index † is nearly equal to that of Canada balsam. Taking flint glass at 1·576,

$$\text{Sin. } \phi' = \frac{1\cdot545}{1\cdot576} \text{ sin. } \phi = 98033 \text{ sin. } \phi,$$

and tabulating a few values for deviation it will be found when the objective is immersed in *turpentine*:—

TABLE IV.

Angle of Refraction from Flint Glass into Turpentine.	Obliquity ϕ of Ray entering Objective.	Deviations.
° ′	°	° ′
0 59	1	0 1
4 54	5	0 6
9 48	10	0 12
19 35	20	0 25
39 4	40	0 56
58 6	60	1 54
74 54	80	5 6
78 37	90	11 23

* Plate glass $\mu = 1\cdot500$. Heavy flint glass $\mu = 1\cdot791$, highly refractive.

† Refractive index: Turpentine, 1·545; C. balsam, 1·532.

Since the quantity of nascent rays or volume of the pencil emanating from the brilliant particle immersed in balsam varies as the square of the angle of total internal reflexion, the results will obtain the following form:—

TABLE V.

Refractions.	Ratio of Quantity of Nascent Pencils transmitted.	Aperture.	Angle of Total Reflexion.
		° ′	° ′
Plate glass to air..	1	83 36	41 48
" water	$2\frac{1}{4}$	125 54	62 57
Flint glass to turpentine ..	$3\frac{1}{2}$	156 46	78 23
Refractions from Cover through Refracting Film.	Quantity of Defining Pencils.	Necessary Objective Aperture.	Limiting Angle of Transmission.

It should be observed here that two special conditions affect the objective definition of a brilliant immersed particle (almost all test objects are mounted in Canada balsam):—

I. *The condition of the rays passing by the particle, glancing as it were past its surfaces and producing the phenomena of diffraction.*

II. *The conditions and affections of the rays nascent from the particle itself acting, when brilliantly illuminated, as a new origin of light, and producing a variety of emergent pencils, themselves affected by the refractive properties of the brilliant particle.*

In the first case, the definition is curiously modified by the illuminating pencils; a slight turn of the concave mirror, for instance, requiring frequently a new cover-adjustment of the screw-collar. And the same principle is recognized by advanced photographers engaged in the instantaneous and enlarging process. Not only do the rays of the ground-glass plate illuminated by the sun affect the picture, but the rays proceeding from the miniature itself. This subtle distinction is of the last importance for understanding both the effect of *nascent* rays emanating from the brilliant particle, and also the condition of the illuminating rays themselves, proceeding from the mirror or condenser.

Col. Dr. Woodward very justly distinguishes between the interference phenomena produced by oblique sunlight without a condenser. A great change, however, in the combined effects of the *nascent* pencils and of diffraction (or glancing) pencils may be produced even by the use of sunlight, provided the condenser have not a larger aperture than 40°, and a stop be placed upon its front glass perforated with a minute orifice of from $\frac{1}{80}$th to $\frac{1}{150}$th of an

inch. Oblique solar rays, whether parallel or not, exaggerate both the slightest chromatic errors of the glasses, unless mono-chromatic rays be employed, as well as diffraction phenomena.

Spherical Aberration Geometrically Displayed.—The comparative *initial* spherical aberrations for *pneumo* and *hydro* objectives or immersion lenses as affecting the nascent pencils, may be conveniently drawn by means of the preceding Tables, on a scale of enlargement one hundred times the real size.

As, however, the deviation between Canada balsam and glass is too small to be delineated (see Table III.), I have neglected it in the accompanying drawings. In each case a nascent pencil proceeding from a brilliant illuminated particle immersed in the balsam, is traced in its deviations for different directions for the *nascent* rays from their origin to their points of immergence into the front surface of the objective, which is here supposed to be plain whatever form of lenses is used for the construction of the front set.

In the water lens the *initial* aberrations denoted by intersections at x_0, x', x_1, x_2, x_3 . . . . are geometrically shown to be very much less than when an air film is employed and denoted by $x_0, x', x_1, x_2, x_3, x_4$ . . . (See Fig. 1 and Fig. 2, Plate LX.)

The adaptation of a "water lens" (which is simply a hemispherical plano-convex lens, of a focal length nearly one-half deeper than the proposed immersion objective) to an old-fashioned combination made on the Lister principle, produces a very inferior objective to the new system adopted by Messrs. Powell and Lealand. Optically speaking a great many plans may be employed to produce a good result. The performance of some of the American and European glasses on the immersion system, as compared with our best English glasses, requires a more thorough investigation and testing than perhaps can be given by Nobert's lines.

I have not entered here upon the question of the comparative dispersive powers of water and glass.* I have not yet been able to ascertain the dispersive power or what is really the kind of glass employed for the "cover." Its influence upon definition cannot be too strongly insisted upon. The chromatic errors of definition (it may here be cursorily remarked) secure rapid transformations from *five distinct sources of change.*

(1.) The difference in the index of refraction of the glass of the "cover" in different specimens.

(2.) The difference in the thickness of the cover, which no

* The subject of chromatic dispersion is reserved for a separate article, if it can be made suitable to this Journal.

ordinary "collar" or objective adjustment thoroughly compensates, when the objective has been nicely balanced to perform its best for one kind and thickness of covering glass.

(3.) The aperture of the nascent pencil actually transmitted.

(4.) The depth at which the observed particle is immersed in the balsam.

(5.) The change in the interval separating the objective lenses.

Such is the subtlety of adjustment required for the successful definition of very difficult objects.

Appendix.

No. 1.—A ray of light having passed out of plate glass ($\mu = 1 \cdot 500$) into air, is striking the plane front of the objective lens at its extreme angle of semi-aperture 85°. What deviation has it suffered in its passage? As the ray is passing from a denser into a rarer medium,

$$\text{Sin. } \phi' = \frac{1}{\mu} \,.\, \text{sin. } \phi,$$

$$\begin{array}{rcr}
\text{or log. sin. } \phi' = \text{log. sin. } \phi - \text{log. } \mu & & \\
\text{log. sin. } 85^\circ \quad .. & = & 9 \cdot 998344 \\
- \text{log. } \mu \text{ (log. } 1 \cdot 500) & = & -\ 0 \cdot 176091 \\
\hline
\text{or log. sin. } \phi' \; .. \; .. \; .. \; .. \; .. & = & 9 \cdot 822253 \\
\text{But log. sine } 41^\circ\ 36' \; .. \; .. \; .. \; .. & = & 9 \cdot 822120 \\
\hline
\text{Difference} \; .. \; .. \; .. & = & 133 \\
\text{Difference for } 60'' \; .. & = & 142
\end{array}$$

$$\text{Hence } 142 : 133 :: 60'' : 56''.$$

The value of ϕ' is therefore 41° 36′ 56″, and as $\phi = 85°$, the deviation required = 43° 23′ 4″, or

43° 23′

nearly, as given in Table I., and

41° 37′

the angle of refraction, as shown in the first and second columns opposite 85, Table I.

No. 2.—For index of refraction between plate glass ($\mu = 1 \cdot 500$) and water ($\mu = 1 \cdot 336$) for mean rays:

$$\frac{1}{\underset{\text{W.G}}{\mu}} = \frac{1 \cdot 336}{1 \cdot 500} = 0 \cdot 890666$$

$$\text{sin. } \phi' = \frac{1}{\mu} \text{ sin. } \phi.$$

$$\text{Hence log. sin. } \phi' = \text{log. sin. } \phi + \text{log. } \frac{1}{\mu}.$$

Suppose it be required to find deviation of a ray entering water from plate glass at an angle of incidence 50°.

Sin. ϕ' .. .. $= 0 \cdot 890666$ sin. $85°$		
Log. sin. ϕ' .. $=$ log. $0 \cdot 890666$ ..		$\bar{1} \cdot 949715$
$+$ log. sin. $85°$		$+ 9 \cdot 884254$
Log. sin. ϕ' .. $=$ sum .. .. .. $=$		$9 \cdot 833969$
Difference for $1' = 135$; log. sin. $43° \, 1' =$		$9 \cdot 833919$
Difference .. .. .. $=$		$0 \cdot 000050$

Hence $135 : 50 :: 60'' : 22''$ nearly.

The value ϕ' required is therefore $43° \; 1' \; 22''$, and as $\phi = 50°$, the deviation $= \phi - \phi' = 6° \, 58' \, 38''$, or

$$6° \; 59'$$

nearly, as given in Table I., in the fourth column opposite 50° obliquity, page 24, July No., M. M. J.

DESCRIPTION OF PLATE LX.

On the Comparative Refractions and Aberration of Radiant Pencils in Immersion Lenses and Common Objectives.

Fig. 1 represents a single plano-convex front of an immersion $\frac{1}{16}$th of Powell and Lealand made in 1869.

The particle Q is supposed to be immersed in Canada balsam in each figure, and protected with a glass cover 0′ 003″ thick.

Radiant rays from Q at angles of incidence 20° 30° 40°, &c., passing through the balsam and glass cover, are incident upon the under surface of the water film, are bent away from the axis Q A, are again deflected towards it, and pass through the substance of the plano-convex in the directions Q A and

$$\left.\begin{array}{l} Q\text{–}0 \\ Q\text{–}1 \\ Q\text{–}2 \\ Q\text{–}3 \\ Q\text{–}4 \\ Q\text{–}5 \\ Q\text{–}6 \end{array}\right\} \text{Cutting the axis in} \left\{\begin{array}{l} x'_0 \\ x_1 \\ x_2 \\ x_3 \\ x_4 \\ x_5 \\ x_6 \end{array}\right\}$$

The points x', x_0, x_1 being in Fig. 1, crowded together at Q, and therefore not delineable.

In Fig. 2 the water film being withdrawn, the primary aberrations or intersections of the refracted rays now cut the axis A Q at much greater distances apart, as shown at $x', x_0, x_1, x_2, x_3, x_4, x_5, x_6$, and the primary aberrations to be corrected by the objective system is at least four times greater for the rays Q R′ or outer shell of the conical radiant pencil, than for a similar shell shown at Q R, Fig. 1.

The utmost aperture transmitted by an air film is also shown at Fig. 2 by the angle A′ Q′ R′, and for the water lens at Fig. 1 by angle A Q R. The course of the rays as they deviate from the straight line during their passage through the various strata represented, is laid down from Table I., calculated in the manner indicated in the appendix.

The angle of total internal reflexion for a ray passing from glass into water, marked as "limiting aperture" in the Plate, is drawn at

$$62° \; 57',$$

and that for a ray passing from glass into air at

$$41° \; 48'.$$

The quantity of *defining rays* radiant from the bright particle Q or Q′ manifestly transmitted, is much greater in the Fig. 1 than Fig. 2: whilst the initial aberrations to be finally corrected are very much less in amount, and of much easier correction or compensation in *immersion than dry objectives*, which doubtless explains the American and European celebrity of the former.

V.—*On the Preparation of Specimens of Soundings for the Microscope.* By Professor A. M. Edwards.

In the course of his gatherings it not unfrequently happens that the microscopist acquires specimens of the bottom of the ocean, from various localities, which, if properly prepared, furnish him with many beautiful objects for observation and study. And the beauty of these objects, the remains of once-living organisms, are of such a marked character and present so many points for admiration, that even to the unscientific observer they become sources of pleasure, often leading to farther inquiry into their life-history, so that imperceptibly almost, and by gradual degrees, the possessor becomes a student in fact, and is induced to follow up his investigations to some practical end.

At the present time such specimens are of especial interest on account of the remarkable revelations made by the deep-sea dredging lately carried on upon our coast and in Europe, and the bearing of these specimens upon Geology, Zoology, as well as other branches of science. The immense tracts covered by what Dr. Carpenter has characteristically termed "Globigerina-mud," on account of it teeming with Foraminifera so called, and the connection of Professor Huxley's "Coccoliths" and "Coccospheres" with the formation of the chalk-beds of the Cretaceous, open up to the microscopists an universe of new facts for investigation. Added to these calcareous organisms the specimens of sea-bottom present us with silicious forms, both animal and vegetable, of surprising delicacy and beauty of outline and structure, and it was to these last-named that my attention has been more particularly turned.

Among such specimens of sea-bottom, the soundings taken at different times and at various points on the coast of the United States by the Coast Survey, have been subjects of extreme value to the scientific observer, while they have, at the same time, furnished many unscientific possessors of microscopes with matter for admiration, wonderment, and joy; the graceful Diatomaceæ, the symmetrical Radiolaria and marvellous Foraminifera often present in such profusion, serving to enable the preparer to put up slides of surpassing beauty.

One of these soundings, for which I am indebted to the Smithsonian Institution, was of such a character that I was extremely desirous of studying as completely as possible the forms presented in it, belonging as they did to all of the three families mentioned, besides which it contained several minute mollusca and the remains of sponges and other organisms. It was, as is shown by the remains present, of such a character as to cor[illegible]tially of Calcium Carbonate, commonly known as Car[illegible]me, and Silicon

Dioxide (Silica), built up into the skeletons of dead organisms. The problem, then, presented to me was to prepare it in such a way, if possible, first, to show all of these objects at the same time, or second, to separate it into calcareous and silicious specimens, and this last method I found, after trial, to be the best. As my mode of manipulation may hereafter be of value to others possessing similar gatherings, I will give it in detail.

The sounding, being in the shape of a dry powder of a light greyish-green colour, was placed in a suitable glass vessel and moderately strong Liquor Potassa poured upon it. It was now boiled for a few moments until I saw that the lumps present in it were broken up and a light mud-like sediment was the result. The solution of Potassa must be, of course, apportioned in strength to the specimen under manipulation, such as consists of many lumps and much organic matter will require it of greater strength than that which is mostly calcareous and silicious. If it be used too strong some of the more delicate silicious forms will be attacked, or even, as I have occasionally found, entirely dissolved. After it had boiled for a short time, as I have said, I allowed it to stand until the mud had settled and a tolerably clear solution was left above it. I now poured off most of the Liquor Potassa and replaced it by a strong solution of Chloride of Soda, so called. That sold by apothecaries under the name of "Labarraque's Solution" will answer generally and is readily procured. This I now boiled until I found its action to cease. By this means the mud is so much bleached as to become almost white. The Potassa at first has the effect of dissolving much of the organic matter present, and thus breaking up the lumps and setting the shells free, and the Chloride of Soda solution bleaches them, so that we have them clean and separated to such an extent that under the microscope the individual shells are easily recognized. I now proceeded to separate the larger from the smaller forms by means of the "Elutriation" process, which consists in first washing off thoroughly all the Potassa and Chloride of Soda with pure filtered or distilled water, and shaking up the sediment in a glass about two inches high filled also with water. If now permitted to stand for a few seconds the larger forms and coarser sand settles, and the supernatent liquid can be poured off into another larger vessel. Again water is added to the first sediment, and, in turn, removed, and this is done as many as six or eight times, until we see that the coarse sediment is not contaminated by finer particles by the water it is shaken in remaining almost clear. The same process is carried out with the sediment in the second vessel, only permitting each charge of water to stand longer than in the first case, as the forms are now much smaller and require longer to settle. In this way we may get three or four densities of sediment, although I found that my specimen

yielded but two which contained anything of interest of a calcareous nature. After, then, setting aside the two first sediments, I carefully acted upon what was left with Hydrogen Nitrate (Nitric Acid) and procured a small quantity of a sediment, consisting for the most part of nothing but the silicious lorica of Diatomaceæ. They were very few, however; so to procure all the remains of Diatomaceæ and other silicious organisms present in the gathering, I took a quantity in the rough state, and, after breaking it down by Potassa, acted on it with boiling Hydrogen Nitrate. Thus I found that I had good representatives of all the gathering contained. Some specimens, however, are not thoroughly cleaned by boiling, even for a considerable length of time in strong Hydrogen Nitrate. To such I add either Hydrogen Chloride alone or a few grains of finely-pulverized Potassium Di Chromate. In this way, and after thoroughly washing with pure water, I have been enabled to obtain extremely beautiful specimens of Diatomaceæ, Radiolaria, and other silicious organisms in a good condition for studying them. This I am engaged upon at the present time and intend before long to lay the results before the Lyceum.—*Paper read before the "Lyceum of Natural History," New York, June 6th.*

VI.—*Circulation of the Latex in the Laticiferous Vessels.*

By H. C. Perkins, M.D.

Within some time I have repeated some experiments (first made more than fifteen years since) upon the circulation of the latex in the laticiferous vessels of the leaf of *Chelidonium majus*, to which I desire to call attention.

Before detailing these experiments it ought, perhaps, to be stated that Amici, Dutrochet, and Mohl deny any visible motion in them except such as is the result of injury; while Schleiden says "that in the uninjured vessels, the motion of the latex can very seldom be successfully shown;" even in *Chelidonium majus* it is only occasionally possible, and then presents great optical difficulties.

Now, I find, by potting a young plant of this kind, and placing any young leaf between two strips of glass (upon which a drop of glycerine has been put) in such a manner as to bring the under side of the leaf uppermost on the stage of the microscope, so as to throw the strong reflected sunlight upon it from the mirror below, that—

First, there is occasionally either a nearly total want of motion or only a very slow one of the coloured granules, or at times a very rapid motion of the particles to be seen, running from right to left, if the vessel happens to run horizontally on the stage, or toward me if the vessel runs from the outer to the inner border of the stage, and

Secondly, that while *watching* the circulation as seen through the lenses in the reflected sunlight, if I move the diaphragm from left to right, so as to make the shadow enter upon the right of the field of view, a brisk circulation (no matter how quiet it had been before) is instantly witnessed, which appears to be changed in direction as we move the diaphragm back again; and that the direction of the *circulation* can thus be *changed* at *will* by the *interception of the sunlight*. This same result can also be witnessed by the passage of clouds between the sun and mirror. The actual direction in the plant is *from* the apex of the leaf *in sunlight* and *toward* it in the shade. This *change in direction* is *so rapid* when produced by the *shadow* of *fast flitting* clouds across the sun's disc that it would seem that the change of temperature could hardly be felt by the plant, it certainly could not be by an ordinary thermometer; but a heated body properly placed will quicken the circulation, as will cold retard it. If I mistake not we have here a fine demonstration of the conversion of light into heat by its passage through the vegetable tissues, and of heat into motion by its action upon the laticiferous vessels.

Prof. Balfour, in the article Botany, 'Ency. Brit.,' says that in plants with milky and coloured juices evident movements have been perceived, and mentions the calyx leaves of *Chelidonium majus*, as also the india-rubber plant, the gutta-percha tree, the dandelion, and the *Euphorbia;* and through your Journal, should you think this article worth insertion, I would ask assistance in the examination of this interesting subject. By mixing a little of the coloured juice with alcohol, and adding a little water, it will be seen that the motion of the liquids in the vessels cannot be the result of evaporation. And that it is not an ocular illusion may be argued from the fact that three independent observers witnessed the changes of motion as above described.

Note, May 12.—I have just examined the circulation of the latex in the laticiferous vessels of *Leontodon taraxacum* under the same circumstances as that of *Chelidonium* and am pleased to find precisely the same results.—*American Naturalist*, July.

VII.—*On a New Species of Parasite from the Tiger.*

By T. Graham Penton, F.Z.S.

Some time ago a small bottle containing some parasites from the tiger was placed in my hands by Mr. Charbonnier, naturalist, of this place, which on examination proved to be a species of Tricodectes which I believe to be hitherto undescribed; in this opinion Mr. H. Denny, to whom I submitted specimens, coincides.

The following is the diagnosis of the species:—

Tricodectes tigris, sp. nov. Pale fulvous, head and thorax fulvous yellow, abdomen pale yellowish white, somewhat quadrate.

Head transverse, vertex concave, with two trapeziform black spots in the centre, lateral margin situated before the eyes, a black spot at the anterior angle passing to the base of the antennæ and uniting with the diagonal fasciæ on each side of the occiput.

Prothorax subconical smaller than the head. Metathorax transverse narrower than head, posterior margin concave ciliated. Abdomen broadly ovate, hairy.

Antennæ, front joint large, subcylindrical; second, smaller; third, clavate and recurved. Legs moderate, tibiæ clavate; ungues slender incurved.

Length, 1·2 millimètre.

CLIFTON, *3rd August*, 1870.

VIII.—*On the Application of the Microscope to the Study of Rocks.*

By H. C. SORBY, F.R.S., &c.

I HAVE read with much interest Mr. Allport's valuable paper "On the Microscopical Structure of Rocks and Minerals," in the 'Monthly Microscopical Journal' for August,* in which he says that the papers by myself and by Mr. David Forbes constitute, he believes, the extent of the published information on the subject. This may be almost true, as far as England is concerned, but is far from being absolutely correct. When I was in Germany, nine years ago, I strongly impressed on my friend Professor Zirkel the importance of the study of the microscopical structure of rocks; and when sitting on the top of the Drachenfels I described to him how thin sections were prepared, and advised him to apply himself to that kind of investigation. I am very glad that he has most successfully carried out my suggestion, and has already published many most excellent papers on the subject. His brother-in-law, Professor Vogelsang, has also taken up the inquiry, and published a separate work, besides some valuable papers. Professor H. Fischer has likewise studied the microscopical structure of minerals, and printed a very useful chronological list of all the papers published in various countries, which are connected with the application of the microscope to the study of mineralogy, petrography, and palæontology.

I here subjoin a list of some of the more important publications, for most of which I am indebted to the authors themselves, which will probably be useful to those interested in the subject.

* Vol. iv., p. 98.

By Professor Zirkel, of Kiel.

1. "Microscopical Investigations of the Glassy and Half-Glassy Rocks."—'Zeitschrift der deutschen geologischen Gesellschaft,' 1867, p. 737.
2. "On the Microscopical Structure of Leucite and of Leucite-bearing Rocks."—*Ib.*, 1868, p. 97.
3. "On the Microscopical Structure of Phonolite." — 'Poggendorff's Annalen,' vol. cxxxi., p. 298.
4. "On the Leucitic Rocks of the Erzgebirge."—*Ib.*, vol. cxxxvi., p. 544.
5. "On the Microscopical Structure of the Lava of Nea Kammeni, near Santorin, erupted in 1866."—'Neues Jahrbuch für Mineralogie, &c.,' 1866, p. 769.
6. "Investigations on the Microscopical Structure of Basaltic Rocks."—A separate work. Bonn, 1870.

By Professor Vogelsang, of Delft.

1. "The Philosophy of Geology, and Studies on the Microscopical Structure of Rocks."—A separate work. Bonn, 1867.
2. "On the Nature of the Liquid enclosed in certain Minerals."—'Poggendorff's Annalen,' vol. cxxxvii., p. 56.

By Professor Fischer, of Freiburg.

1. "A Chronological Survey of the gradual Introduction of the Microscope into the Study of Mineralogy, Petrography, and Palæontology."—A separate pamphlet. Freiburg, 1868.
2. "Critical Microscopical Mineralogical Studies."—A separate pamphlet. Freiburg, 1869.

By A. Kengott.

"Observations on Thin Sections of a Caucasian Obsidian."—A separate pamphlet. St. Petersburgh, 1869.

IX.—*On the Focal Length of Microscopic Objectives.*

By Chas. R. Cross.

The investigation of which the present article is a summary, was undertaken in order to see if some reliable method of measuring the focal length of microscope objectives could not be found. The importance of such a method will be apparent to all who have had occasion to make use of objectives by different makers. The focal length of lenses of the same denomination is subject to so great a

variation, that comparison of these by means of their assumed focal lengths too often gives no true idea of their relative excellence. For example, if two quarter-inch objectives be compared, and one gives results much superior to that given by the other, we cannot be at all sure that the better lens is not really of shorter focus than its designation would indicate.

The question immediately arises, what is the focal length of a compound objective? The focal length of a simple lens, or of a system of lenses in actual contact, is the distance from the optical centre of that lens or system to its principal focus. But as a system of lenses not in contact, like the triplet objective, has no optical centre, the term is only a general appellation serving to group together objectives of approximately the same magnifying power. If every system of lenses possessed an optical centre, or could be replaced by a single lens, we might define the focal length of such a system as being the focal length of a single lens equivalent to the system in magnifying power. But as this is not the case, a lens replacing the system when the conjugate foci are separated from each other by a distance, d, will not replace it if the foci are separated by any other distance, d^1; and the difference in focal length of lenses replacing the system under these different circumstances varies with the internal arrangement of the system. But for any constant distance between the conjugate foci any system can be replaced by an equivalent single lens; and in order to attach a definite meaning to the term "focal length," as applied to triplet objectives, I would propose that the focal length of such an objective be understood as being the focal length of a lens replacing the system when the distance between the conjugate foci is 10 inches.

The present series of experiments was begun upon the supposition that a triplet microscope objective has an optical centre, and the formulæ applied to obtain the focal length were based on this supposition, which, on further investigation, proved to be incorrect. Observations recorded in the sequel showed, however, that though not exactly correct, the formulæ offer a very close approximation to the truth for distances of about 10 inches between the conjugate foci; the variation in the focal length of the lens replacing the system corresponding to a difference of several inches in that distance being very small.

We may, then, deduce a formula by which to find an equivalent simple lens from the relations between the principal focus, the conjugate foci, and the relative magnitude of object and image for given distances between them.

The principal focus of a simple lens can be determined from the formulæ, $\frac{1}{p} + \frac{1}{p^1} = \frac{1}{f}$, in which p and p^1 are the conjugate foci

obtained by direct experiment, and f is the principal focus sought. Even if the triple objective had an optical centre, this formula could not be applied directly, owing to the practical impossibility of finding this centre, which, moreover, would change with the relative change of position of the lenses composing it in varying the adjustment for different thicknesses of the glass covering the object. The direct use of the formulæ would require the distances p and p^1 to be known. Some modification must, therefore, be adopted.

The method ordinarily in use by the makers seems to be to grind the lenses with certain radii, which are assumed to give approximately definite "focal lengths." The glasses, if tested at all, are compared with some standard objective, by means of their magnifying powers with the same eye-piece, a method liable to considerable errors.

The method which I use is based upon two equations, the one given above, $\frac{1}{p}+\frac{1}{p^1}=\frac{1}{f}$ (1) and $\frac{p}{p^1}=\frac{\text{size of object}}{\text{size of image}}=n$ (2), in which n is the ratio of the size of the object to the size of the image, the conjugate foci being p and p^1. It is clear that though we cannot measure p and p^1 separately, we can measure their sum, that is the distance between the object and the image, which we call l; n can be found by measuring the size of the image of an object of known magnitude, a finely-divided scale throwing this image on a second divided scale.

We have from (1) $p^1f+pf=pp^1$, or $lf=pp^1=(l-p^1)\,p^1$ (3), as $p+p^1=l$. But $p=np^1$ from (2); therefore, $p^1+np^1=l$, and $1^1=\frac{l}{n+1}$ (4). Combining (3) and (4) $fl=\frac{nl^2}{(n+1)^2}$, and $f=\frac{nl}{(n+1)^2}$. If, therefore, we find n and l, we can easily find f, the focal length of the equivalent lens.

Two sets of experiments were made with a somewhat different arrangement of apparatus in each. In both the image of a glass scale divided to $\frac{1}{100}$ millimètres ($\frac{1}{2500}$ inch) was thrown upon an engine-divided paper scale ($\frac{1}{50}$ or $\frac{1}{60}$ inch), the image and the paper being the focus of a double-convex lens used as an eye-piece, so that the size of the magnified image was read directly on the paper scale, estimating by the eye to tenths of the divisions. The distance from the glass to the paper was measured with a steel rule graduated to millimètres. The magnitude of the image varies so slowly for any variation of l that this was taken only in whole millimètres. Any error in the measurement would be less perceptible in the results, the shorter the focus of the lens measured.

The first form of apparatus consists of a stand with a vertical block pierced with two holes, in one of which is placed the objective to be measured with its optical axis horizontal. Through the other hole (the lower one) slides a horizontal bar, at one end of which is the micrometer used as the object, at the other end the paper scale on which the size of the image is measured, reading with an eye-piece detached from the instrument. This bar is moved in either direction until the image is thrown directly on the paper scale, the motion being accomplished by a screw giving a fine adjustment, while the nut in which the screw plays is moved by the hand for the coarse adjustment. The length, l, between the two scales was measured once for all.

The results given by this arrangement were very satisfactory, but owing to its horizontal position and a difficulty in centring the lenses, I afterwards adopted a somewhat different apparatus, with which most of the recorded observations were made. Here I employ a microscope stand, using simply the tube and stage, placing the objective in its usual position, and throwing the image of the micrometer upon the paper scale. The latter is now gummed to the end of the tube usually occupied by the eye-piece, which is replaced by a convex lens detached from the instrument as before. The distance, l, now a variable, has to be measured with each objective. This arrangement can evidently be applied to all microscopes, rendering it possible for anyone to determine the focal length of his own objectives with the greatest ease.

The following measurements will show the accuracy of the method:—

Tolles, Second Quality, ¼.				Smith and Beck, ¼.			
Apparatus No. 1.		Apparatus No. 2.		Apparatus No. 1.		Apparatus No. 2.	
$l = 311$ mm.		$l = 246$ mm.		$l = 311$ mm.		$l = 253$ mm.	
f.	N.	*f.*	N.	*f.*	N.	*f.*	N.
·2416	20	·2424	30	·2115	20	·2103	40
·2416	10	·2419	50	·2112	30	·2097	40
·2416	20	·2415	50	·2107	30	·2097	40
·2397	20	·2415	40	·2107	30		
M. = ·2411		M. = ·2418		·2107	30	M. = ·2099	
P. E. = ·00029		P. E. = ·00011		·2107	30	P. E. = ·00011	
p. e. = ·00058		p. e. = ·00022		·2104	25	p. e. = ·00019	
				·2086	20		
				·2086	20		
				·2079	20		
				M. = ·2095			
				P. E. = ·00025			
				p. e. = ·00083			

The preceding measurements were made with both forms of

apparatus, and at an early period of the investigation; the following are by the second method alone, and were taken after considerable practice in using the apparatus.

Hartnack, No. 9.				Tolles, $\frac{1}{15}$.				Natchet, No. 7.			
Adjusted for				Adjusted for				Adjusted for			
Object Covered.		Object Uncovered.		Object Covered.		Object Uncovered.		Object Covered.		Object Uncovered.	
$l = 248$ mm.		$l = 249$ mm.		$l = 257$ mm.		$l = 258$ mm.		$l = 265$ mm.		$l = 265$ mm.	
f.	N.	*f.*	N.	*f.*	N.	*f.*	N.	*f.*	N.	*f.*	N.
·0819	10	·0917	10	·0640	10	·0900	15	·0721	10	·0898	15
·0816	10	·0911	10	·0636	10	·0898	15	·0721	10	·0898	15
·0816	10	·0910	10	·0636	10	·0898	15	·0718	10	·0897	10
M. = ·0817		·0910	15	M. = ·0638		M. = ·0899		·0718	10	·0896	15
P. E. = ·00006		M. = ·0912		P. E. = ·00001		P. E. = ·00004		M. = ·0719		M. = ·0897	
p. e. = ·00010		P. E. = ·00009		p. e. = ·00002		p. e. = ·00007		P. E. = ·00005		P. E. = ·00003	
		p. e. = ·00019						p. e. = ·00010		p. e. = ·00006	
$l = 249$ mm.											
f.	N.	$l = 264$ mm.									
·0817	10	*f.*	N.								
·0817	10	·0923	10								
·0817	10	·0914	10								
·0817	10	·0911	10								
·0816	10	M. = ·0915									
M. = ·0817		P. E. = ·00020									
P. E. = ·00002		p. e. = ·00034									
p. e. = ·00006											

In the two preceding Tables the focus f is given in decimals of an inch, as computed from each reading. N is the number of spaces observed on the micrometer scale, and l the distance between the two scales. The most probable mean M is computed by giving weight to each observation proportional to the number of spaces N taken; that is, assuming that the whole error lies in reading the size of the magnified image on the paper scale. P. E. is the probable error of the mean M, and *p. e.* that of one observation.

In all the measurements the objective was refocussed for each reading, and in those given above it was dismounted and remounted for each set.

The Table below gives the result of several hundred measurements on various objectives. The first column gives the order in the Table for convenience of reference; the second the name of the maker and designation as given by him; the column headed A gives the adjustment, whether for covered or uncovered glass, with the position of the index, if this is present; also whether the lens is wet or dry if an immersion lens. The next column gives in millimètres the distance denoted in the formula by l; that is, the distance between the two scales. The value of f is in all cases the

most probable mean of a number of observations. The column headed B gives the focal length indicated by the maker in decimals of an inch. The next column, headed "diff. A," gives the difference between the actual focal length, as determined by these observations, and the designated focal length as given in column B. The last column, headed "diff. B," gives the difference in decimals of an inch between the *extreme* value of f as given from different observations by this method.

The micrometer used as an object was a glass ruled scale divided to $\frac{1}{100}$ mm. ($\frac{1}{2500}$ inch), except in the objectives numbered 9, 10, 16, 18, 28, 31, 32 (marked with an asterisk), in which a $\frac{1}{1000}$ inch stage micrometer was substituted, as the more finely divided scale could not be read with such low powers as these objectives gave.

Objectives Nos. 16 and 28 are single lenses; the rest are triplets. Lens No. 32, by Zentmayer, had been slightly altered in focal length to adapt it to a gas microscope. The adjustment for covered objects shortens the focus of all the glasses examined, except Nos. 11, 12, and 17, in which a lengthening of focus takes place. No. 11 is adjusted for glass covering when the index is at o, contrary to the usual method in which the greater the number of the index the thicker the covering for which the objective is adjusted. Objective No. 12 is No. 11 with the front lens removed and an "immersion front," a lens of different focus screwed on in its place.

No.	Name of Maker.	A.	l.	Focal Length.		B.	Diff. A.	Diff. B.
			mm.	mm.	in.	in.	in.	in.
1 a	Hartnack, No. 10 .	Unc. Dry	249	1·985	·0782	..	..	·0006
b	.. .. ..	Cov'd Dry	249	1·696	·0668	..	..	·0009
c	.. .. ..	Unc. Wet	249	1·991	·0784	..	..	·0002
d	.. .. ..	Cov'd Wet	249	1·681	·0662	..	..	·0002
2	Hartnack, No. 9 ..	Unc. Dry	249	2·317	·0912	..	..	·0007
		Unc. Dry	264	2·325	·0915	..	..	·0012
		Cov'd Dry	248	2·079	·0818	..	..	·0003
		Cov'd Dry	249	2·073	·0816	..	..	·0001
		Cov'd Dry	261	2·079	·0617	..	..	·0006
		Unc. Wet	261	2·339	·0921	..	..	·0007
3	Hartnack, ¼ in. ..	..	249	6·295	·2478	·2500	−·0022	·0013
4	Hartnack, ½ in. ..	..	259	13·659	·5377	·5000	+·0377	·0027
5 a	Nachet, No. 7 ..	Unc'd ..	265	2·279	·0897	..	..	·0002
b	.. .. ..	Cov'd ..	265	1·827	·0719	..	..	·0003
6	Nachet, No. 5 ..	..	264	3·066	·1207	..	..	·0004
7	Nachet, No. 3 ..	..	235	4·666	·1758	..	..	·0004
8	Nachet, No. 2 ..	..	239	6·469	·2547	..	..	·0006
9*	Nachet, No. 1 ..	..	244	14·791	·5823	..	..	·0012
10*	Nachet, No. 0 ..	..	268	39·103	1·5395	..	..	·0027
11 a	Ross's $\frac{1}{12}$ in.	0 ..	249	1·675	·0659	..	..	·0004
b	.. .. ..	22 ..	250	1·900	·0748	·0833	−·0085	·0013
12 a	Ross's $\frac{1}{12}$ in., with immersion front by Tolles	0 Dry ..	249	1·265	·0498	..	..	·0001
b	.. .. ..	0 Wet ..	249	1·273	·0501	..	..	·0006

TABLE—*continued.*

No.	Name of Maker.	A.	l.	Focal Length.		B.	Diff. A.	Diff. B.
			mm.	mm.	in.	in.	in.	
c	.. .. ..	31 Dry ..	250	1·696	·0668	..	..	·0005
d	.. .. ..	31 Wet ..	250	1·689	·0665	..	..	·0006
13	Smith & Beck, $\frac{1}{5}$ in.	..	255	4·906	·1931	·2000	−·0069	·0017
14 a	Smith & Beck, $\frac{1}{4}$ in.	0 ..	253	5·332	·2099	·2500	−·0401	·0006
b	.. .. ..	18 ..	253	5·038	·1983	..	..	·0005
15	Smith & Beck, $\frac{2}{3}$ in.	..	257	16·330	·6429	·6667	−·0238	·0000
16*	Smith & Beck, $1\frac{1}{2}$ in.	Single lens	276	33·990	1·3382	1·5000	−·1617	·0033
17 a	Collins, $\frac{4}{10}$ in. ..	3 ..	258	8·180	·3220	..	..	·0009
b	.. .. ..	20 ..	259	8·502	·3347	·4000	−·0653	·0010
18*	Collins, $1\frac{1}{2}$ in. ..	..	271	42·572	1·6761	1·5000	+·1761	·0047
19 a	Spenser, $\frac{1}{2}$ in. ..	Unc'd ..	266	9·717	·3826	·5000	−·1174	·0000
b	.. .. ..	Cov'd ..	266	8·947	·3522	..	..	·0018
20 a	{Tolles, "$\frac{1}{12}$ in. dry, $\frac{1}{15}$ in. wet" ..}	0 Dry ..	258	2·286	·0899	·0833	+·0066	·0002
b	.. .. ..	19 Dry ..	257	1·620	·0638	..	..	·0004
c	.. .. ..	6·3 Dry ..	256	2·024	·0797	..	..	·0012
d	.. .. ..	6·3 Wet .	256	1·995	·0786	·0667	+·0119	·0003
21	Tolles, $\frac{1}{12}$ in. ..	0 Dry ..	257	2·213	·0871	·0833	+·0038	·0011
22	Tolles, $\frac{1}{10}$ in. ..	0 Dry ..	257	2·898	·1141	·1000	+·0141	·0014
23 a	Tolles, $\frac{1}{10}$ in. ..	0 Dry ..	258	2·874	·1131	·1000	+·0131	·0000
b	.. .. ..	14 Dry ..	259	2·283	·0899	..	..	·0008
c	.. .. ..	0 Wet ..	258	2·910	·1146	·1000	+·0146	·0005
d	.. .. ..	14 Wet ..	259	2·263	·0891	..	..	·0004
24 a	Tolles, $\frac{1}{8}$ in.	0	258	3·217	·1267	·1250	+·0017	·0011
b	.. .. ..	$16\frac{1}{2}$	258	2·683	·1056	..	..	·0005
25 a	Tolles, $\frac{1}{4}$ in.	0	263	6·083	·2395	·2500	−·0105	·0011
b	.. .. ..	10	263	5·675	·2234	..	..	·0026
26	Tolles, $\frac{1}{4}$ in., 2nd Q.	..	246	6·143	·2418	·2500	−·0082	·0009
27	Tolles, $\frac{1}{4}$ in., 2nd Q.	..	248	6·400	·2520	·2500	+·0020	·0010
28*	Tolles, 1 in., 2nd Q.	Single lens	259	25·188	·9916	1·0000	−·0084	·0000
29 a	Wales, $\frac{4}{10}$ in. ..	0	258	6·818	·2684	·4000	−·1316	·0008
b	.. .. ..	9	258	6·563	·2584	..	..	·0007
30	Zentmayer, $\frac{4}{10}$ in. ..	..	255	9·193	·3619	·4000	−·0381	·0009
31*	Zentmayer, $\frac{8}{10}$ in. ..	..	269	20·113	·7918	·8000	−·0082	·0000
32*	Zentmayer, $1\frac{1}{2}$ in. ..	..	280	36·839	1·4504	1·5000	−·0496	·0022
33 a	Grunow, $\frac{1}{4}$ in. ..	Unc'd ..	273	5·253	·2068	·2500	−·0432	·0012
b	.. .. ..	Cov'd ..	273	5·059	·1992	..	..	·0003
34	Grunow, $\frac{1}{2}$ in. ..	..	255	8·902	·3505	·5000	−·1495	·0021

The contents of column A may need some further explanation. The immersion lenses were measured both wet and dry to determine the change of focal distance, which is very small unless the interior arrangement of the lenses is altered by moving the index circle. The figures in this column are the numbers given by the index if there was one; if no index was attached to the objective the extremes of the adjustment were taken, and are indicated by the words "Unc." for uncovered, and "Cov'd" for covered adjustment. The $\frac{1}{100}$ millimètre scale used as the object was always uncovered, whether the lens was adjusted for covered and uncovered objects, as no other method seemed so generally applicable. This, of course, rendered the definition somewhat indistinct when the lens was

adjusted for glass covering, which will explain the greater difference in the corresponding extreme values of f in the last column.

The measurements in the preceding Table were made with the second form of apparatus, so that the length of l varies slightly from the normal length of 10 inches. The extreme values are 276 mm. (10·87 inches) for No. 16, and 235 mm. (9·25 inches) for No. 7. To ascertain the effect of this variation on f, the computed focal distance, the following observations were made. 1st. A Smith and Beck $\frac{1}{4}$ inch (No. 14) was measured, first with $l = 279$ mm. (10·98 inches), and then with $l = 412$ mm. (16·22 inches). The computed values of f in these two cases were—

$$l = 279 \text{ mm.}, f = \cdot 2102 \text{ inch}; \; l = 412 \text{ mm.}, f = \cdot 2035 \text{ inch},$$

giving a difference of only ·0067 inch in f for a difference of 133 mm. (5·24 inches) in l. 2nd. A Tolles second quality $\frac{1}{4}$ inch (No. 26) was measured in the same way, giving values of f as follows:—

$$l = 259 \text{ mm.}, f = \cdot 2424 \text{ inch}; \; l = 414 \text{ mm.}, f = \cdot 2395 \text{ inch},$$

a difference of but ·0029 inch, corresponding to a difference of 155 mm. (6·10 inches) in l. From these results it was inferred that with the maximum deviation (in No. 16) of 22 mm. (0·87 inch) from the normal value of l, the correction required to reduce the value of f to that standard length would be within the limits of probable error, and in most of the objectives the deviation of l is far less than in this case.

An examination of the Table will show that the focal length of the objectives of some makers differs considerably from the length marked upon them. For example, No. 34 marked $\frac{1}{2}$ inch is really a $\frac{1}{3}$ inch objective; No. 33 marked $\frac{1}{4}$ inch is really a $\frac{1}{5}$ inch; No. 29 marked $\frac{4}{10}$ inch is a $\frac{1}{4}$. Lens No. 14 marked $\frac{1}{4}$ inch is really a $\frac{1}{5}$ inch; but Nos. 13, 15, by the same makers, are correctly designated $\frac{1}{5}$ inch, $\frac{2}{3}$ inch. Differences of this kind must of necessity lead to a great confusion in comparing objectives with one another. I would therefore suggest that each objective made should be measured before being offered for sale, that this confusion may cease to exist. A convenient arrangement would be to fix a glass scale divided to $\frac{1}{50}$ or $\frac{1}{100}$ inch in the draw-tube, sliding in the tube of the microscope, and measure as I have already described. The draw-tube should be moved till the front of the ruled glass is exactly 10 inches from the micrometer used as the object. Or it would be more convenient still to have an apparatus similar to the first form, but arranged with a suitable stage and stand so that it can be set at any desired angle. The distance 10 inches (254 mm.) suggested as a standard is chos[illegible] it is the normal distance

of distinct vision, as well as about the length used by microscopists in actual work.

An inspection of the formula $f = \frac{n\,l}{(n+1)^2}$ shows (1) that the focal length of any lens is not inversely proportional to its magnifying power with a given distance l between the conjugate foci, as is commonly assumed, but to $\frac{n}{(n+1)^2}$, the ordinary supposition approaching absolute correctness as n increases. Hence the inaccuracy of any system of estimating focal lengths upon this assumption when applied to lenses of long focus.

(2.) The shorter the focal length of the objective, the less will any error in the measurement of l affect the result.

(3.) Any error in the measurement of n also affects the result less in a lens of short focus. It would therefore appear that by this method the most accurate results are obtained with the objectives of highest power. The following examples from the records of my observations will illustrate this last point. The numerators of the fractions are the readings on the paper scale, the denominators, the number of spaces of the micrometer scale corresponding to these readings, the quotient being of course the value of one of these magnified spaces in fiftieths of an inch.

With objective No. 16*, Smith and Beck, $1\frac{1}{2}$ inch—

Scale readings

$$\frac{32\cdot8}{11} = 2\cdot982. \quad \frac{32\cdot7}{11} = 2\cdot973. \quad \frac{32\cdot7}{11} = 2\cdot973.$$

Give values of $n = 5\cdot964. \quad 5\cdot946. \quad 5\cdot946.$

The corresponding values of f are—

1·3362 inch. 1·3392 inch. 1·3392 inch.

$l = 276$ mm. M $= 1\cdot3382$ in. Difference of extremes $= \cdot0030$ in.

With objective No. 8, Natchet No. 2—

Scale readings

$$\frac{41\cdot2}{60} = \cdot6866. \quad \frac{41\cdot2}{60} = \cdot6866. \quad \frac{34\cdot4}{50} = \cdot6880. \quad \frac{41\cdot3}{60} = \cdot6883.$$

Give values of $n =$

34·879. 34·879. 34·950. 34·965.

The corresponding values of f are—

·25494 inch. ·25494 inch. ·25446 inch. ·25435 inch.

$l = 239$ mm. M $= \cdot25466$ in. Difference of extremes $= \cdot00059$ in.

With objective No. 1, *c*, Hartnack, No. 10—

Scale readings

$$\frac{36\cdot4}{15} = 2\cdot447. \quad \frac{24\cdot2}{10} = 2\cdot420. \quad \frac{36\cdot3}{15} = 2\cdot420. \quad \frac{36\cdot3}{15} = 2\cdot420.$$

Give values of n =

123·29. 122·94. 122·94. 122·94.

The corresponding values of f are—

·07824 inch. ·07845 inch. ·07845 inch. ·07845 inch.

l = 249 mm. M = ·07839 in. Difference of extremes = ·00021 in.

The increasing change in f for the same variation of the scale reading is clearly seen on comparing the above sets of observations. The diminution of the number of divisions measured in a short-focus objective is a partially neutralizing circumstance, which can, however, be avoided by using a lens of long focus for the eye-piece, so as to gain a larger field of view.

The chief difficulty met with in pursuing this research was that of procuring a suitable scale for the object, the image of which was to be measured. In the earliest measurements a scale on glass ruled to $\frac{1}{1000}$ inch was used, but the lines were jagged at the edges, their breadth was variable and their spacing unequal. Next, an eye-piece micrometer belonging to a Smith and Beck's microscope was used, the divisions being $\frac{1}{200}$ inch, but though this was an improvement on the former, the results were still unsatisfactory. Finally, a micrometer reading to $\frac{1}{100}$ mm. was used, which was all that could be desired in clearness and evenness of lines and equality of spacing. In some cases, for long-focus objectives a $\frac{1}{1000}$ inch micrometer scale was substituted, as before stated. The paper scale used to measure the size of the image was divided to $\frac{1}{50}$ or $\frac{1}{60}$ of an inch, while these divisions were sub-divided by the eye into tenths, giving the measurement to $\frac{1}{500}$ or $\frac{1}{600}$ of an inch. The measurements can in general be relied upon within $\frac{1}{500}$ or $\frac{1}{600}$ of an inch. A scale divided into fractions of a millimètre would have been preferable had such been easily procurable, as saving labour in the calculation of the focal lengths.

Difficulty was at first apprehended from the expansion of the paper scale, especially as this was attached to the microscope tube by mucilage, but measurements made at different times both when moist and when dry showed no appreciable variation to be ascribed to that cause. A steel scale was at one time substituted, but being less easily read, its use was abandoned. A glass scale divided to half millimètres would be valuable in an extensive application of this method, because of the greater ease of estimating to tenths with narrow lines, those on printed scales being necessarily somewhat coarse.

The present investigation has suggested an inquiry into the laws of the foci of systems of lenses as related to variations in the distances between them, which, in connection with the law of variation of focal length with varying values of l, it is my intention to make the subject of further research.

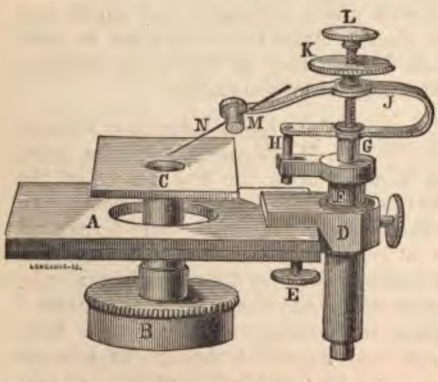
I would take this opportunity of expressing my obligations to

the gentlemen who have so kindly allowed me the use of their objectives, and especially to Mr. Charles Stodder, of the Boston Optical Works, for the large number of triplet lenses, by various makers, which he has placed at my disposal.

It is hoped that the method described in this paper may be found of value as offering a ready and reliable method of measuring focal lengths, so that there may be no necessity of doubt as to the true focal length of any objective.—*Contributions from the Physical Laboratory of the Massachusetts Institute of Technology.*

PROGRESS OF MICROSCOPICAL SCIENCE.

A New Mechanical Finger for the Microscope.—Mr. J. Zentmayer gives the following description of a recent invention by him in the 'Journal of the Franklin Institute' for May:—In the study of diatoms it has been long desired to find a substitute for the clumsy fingers of the human hand to do the delicate work of picking up rare and valuable diatoms detected by the microscope, and to transfer them to a glass slide for preservation. Prof. H. L. Smith, well known by microscopists as the inventor of several valuable accessories to the microscope, first presented us with a very ingenious little instrument of that kind. Messrs. John H. B. Latrobe and George Dobbin, of Baltimore, two expert microscopists, had in use for some time one of these instruments, but found it difficult to work it; and as the instrument was exceedingly well made, this proved that its construction was too complicated to give the firmness required in picking up a shell less than the thousandth part of an inch in length, and of which a single ounce of ocean sand contains, sometimes, many millions. Mr. Latrobe invited me to design and construct for him an instrument for this purpose. The instrument requires many adjustable movements, and each of these increases its liability to shake and spring. So I made it my object to utilize such movements of a first-class microscope stand as are not essential for other specific operations, as parts of the new finger. I found in the movements of a mechanical or sliding stage the main movements required in the finger, and so attached the apparatus to the mechanical stage. This gave me two of the most important movements, with a firmness and with dimensions of parts for which, otherwise, there would be no room. This step, however, made it necessary to provide for another stage; but as there is never a higher power than a $\frac{2}{3}$rds employed with such an apparatus, a plain stage with some simple arrangement to hold the slides would be found quite sufficient—such a one was therefore arranged. The cut represents the finger attached to one of my large microscopes. A is the top plate of the mechanical stage; the circular plate is omitted. The cap B is fitted to the lower body below the stage, into which cap the new sub-stage C is fastened by a narrow tube, wide enough to admit illumination from the mirror. As the lower body is movable up and down by a rack, another movement is gained which is necessary to accomplish our result. The difference



of the size of the aperture of the stage and the diameter of the tube which connects the sub-stage with the cap A is equal to the movement of the mechanical stage, and this is found more than sufficient. D is the clamp by which the finger is attached to the stage by means of the screw E. A steel cylinder G is nicely fitted into the top and bottom of the tube F, leaving room inside for a light spring to press the steel cylinder upwards. To prevent turning, the spring J is provided, at H, with a steel pin accurately fitted into the fork at the top of the tube F. By turning the nut K the spring J is elevated and depressed, giving nice adjustment to the needle N, in case the finger is to be attached to a microscope not having rack-movement to the cap B, to bring the end of the hair and the object in close approximation. The end of the spring J forms a little ring, with a screw cut inside, into which a cork M is screwed to receive a needle N, to which a hair is fastened by wrapping gum-paper around. Turning the cork facilitates the adjustment of the hair to the proper inclination. A slight pressure on the button L brings down the hair, and the spring, inside of F, instantly lifts it again when the pressure is removed. The tube F turns in the clamp D in order to adjust the hair and cork more conveniently, and when brought back again it is tightened by a set screw. Complicated as it may appear, only one movement is added to the microscope stand by this instrument, the one, *viz.* which gives the vertical motion. When the apparatus is to be used the material you want to select from is placed on the sub-stage C and focussed, then the point of the hair is approximately brought over the selected object by means of the stage movements and turning of D; this brings the hair nearly in focus too, because it is almost in the same plane with the object. We next adjust the hair precisely over the selected shell, press down the button L, and the shell will adhere to the hair. Now we remove the slide with the material and substitute a glass slide, moistened by breathing on it, and having brought it in proper position, briskly dip down the button L again and the shell will be deposited on the glass slide. As the mechanical stage has a graduated indicator, the finger may be moved along regularly, and shells may be placed at equal distances in lines. After the cover-glass is carefully placed over it, then Canada balsam may be run in by capillary attraction without disturbing the position of the shells.

Report upon a Specimen of Anemone nemorosa infested by a Fungus. —This fungus is, according to Prof. A. M. Edwards, a species quite common both in America and Europe, upon the true leaves of the *Anemone* in early spring, and has been named *Puccinia Anemones.* In Ray's 'Synopsis'* it is described in company with true ferns, and was for a long time supposed that the deeply-cleft leaf of the *Anemone* with the brown spots upon its under side was a fern with sori. As Ray says, "this capillary was gathered by the Conjuror of Chalgrave," hence it has come to be known as the Conjuror of Chalgrave's fern. This fungus, like the other microscopic parasitic ones, grows beneath the surface of the plant, throwing out its threads of mycelium among

* 3rd edition, 1724.

the cells, until it develops the brownish-coloured bodies known as *spores* (perhaps incorrectly), and it is by the peculiar characters of these that species have been distinguished; although there seems two good reasons for supposing that these plants are not only dimorphous, as has been stated, but polymorphous, assuming different forms according to the habitat in which they are found. In reply to the question as to whether it was true, as was stated by farmers, that Barberry bushes infested with fungus, or mildew, conveyed that mildew to fields of wheat adjoining, which then showed the presence of "brand," Prof. Edwards remarked that such might very likely be the case, as very little certain is known respecting the life history of these minute plants, and he was now carrying on some experiments, by infesting different plants with fungi taken from others, so as to see if the host which they inhabited modified their characters materially. He described, and illustrated by means of diagrams, the characteristics of the Wheat Brand, *Puccinia graminis*, and other fungi, and expressed a hope that botanical readers would contribute specimens of such plants as they found to be infested by mildews, brands, and smuts for the Society's collection.

Position of the Brachiopoda in the Animal Kingdom.—For some time past Mr. Edward S. Morse has had reasons for believing that the Brachiopods with the Polyzoa, had greater affinities with the worms than with the mollusks. He has studied attentively *Terebratulina* and *Discina* as well as their early stages, and in all points of their structure interprets articulated characters, and not molluscan characters. Without entering into particulars at this time, he would state that in the structure of the shell he finds the greatest resemblance to the shell of crustacea, both as regards the peculiar tubular structure, and the scale-like appearance, and its chemical composition. In *Lingula*, while the carbonate of lime amounts to only six per cent., the phosphate of lime amounts to forty-two per cent. The horny setæ which fringe the mantle are remarkably worm-like. In worms the bristles are enclosed in muscular sheaths, while in other articulate animals the hairs are simply tubular prolongations of the epidermal layer. In the Brachiopods these bristles are secreted by follicles and are surrounded by muscular fibres, and are freely moved by the animal. The structure of these setæ differs but little, if at all, from those of the worms. The lophophore with the cirri is to be compared to similar parts in the tubicolous worms, and the mantle which covers and conceals their arms is to be compared to the cephalic collar, as seen in Sabella, for instance, where we find it split laterally, and a portion reflected. If this were greatly developed so as to cover the expanded fronds of cirri, we should recognize quickly the relation between the two. Dr. Gratiolet has compared the circulatory system of the Brachiopods to that of the crustacea, and Burmeister has shown a resemblance between the respiratory apparatus of certain cirripeds and that of Lingula. In the reproductive system there is a close similarity existing between the oviducts of Brachiopoda, with their trumpet-shaped openings, and similar organs in the worms. In the little knowledge we have of their embryology, the strongest proofs exist of

their affinity with the worms. Lacaze-Duthiers figures the embryo of Thecidium, and it is a little animal with four segments. Fritz Müller figures an early stage of Discina, and we have recalled to us a positive articulate and worm-like character. From the body of this embryo prominent bristles project. Smitt figures the same in the embryo of Lepralia, wherein he describes six bristles that appear locomotive; and Claparède figures the embryo of Nerine, a worm, in which we find similar bristles projecting from the body. In this connection it is interesting to note that in the winter eggs, or statoblasts, of Polyzoa we have a relation to similar characters among the lower crustacea, the ephippia of Daphnia and the winter eggs of Rotifers for example. Leuckart places the Polyzoa with the worms, and the close affinity of the Polyzoa with the Brachiopoda is now freely admitted, and we now recall those peculiar worms, or early stages of them, which so strongly resemble in almost every essential point of their structure the hippocrepian Polyzoa. As many of the foregoing points need ample illustration, and as the writer has in preparation a memoir on the subject, he will now only call attention to the facts supporting these views, evolved from the study of living Lingulæ. It is but justice to state that six months previous to the observations made on Lingula, he had come to conclusions herein expressed, and had freely argued it with his colaborators. He saw the necessity of examining Lingula, however, before advancing these views, and for this sole purpose had visited North Carolina in company with Dr. A. S. Packard, jun., who with his observations on the worms and crustacea of that region yet found time to follow the writer, step by step, in his studies of Lingula, and was deeply impressed by the disclosures there made. His sincerest gratitude is due to Dr. Elliott Coues, U.S.A., and Major Joseph Stewart, U.S.A., commandant at Fort Macon, North Carolina, for their constant aid and sympathy in furtherance of the object of his visit there. After nearly a week's fruitless search, Lingulæ were found in a sand shoal, left at low tide. They were found buried in the sand. The peduncle, which was about six times the length of the shell, being encased in a *sand tube* differing in no respect from the sand tubes of neighbouring annelids. In many instances the peduncle was broken in sifting them from the sand, yet the wound was quickly healed and a new sand-tube promptly formed. When placed on the surface of the sand they were noticed to move quite freely, by the sliding motion, in all directions, of the dorsal and ventral plates, aided at the same time by the rows of setæ or bristles, which swung back and forth like a galley of oars, leaving a peculiar track in the sand. The peduncle was hollow, and the blood could be seen coursing back and forth in its channel. It was distinctly and regularly ringed, and presented a remarkably worm-like appearance. It had layers of circular and longitudinal muscular fibre, and coiled itself in numerous folds or unwound at full length. It was contractile, also, and quickly jerked the body beneath the sand when alarmed. But the most startling discovery in connection with this interesting animal was the fact that its blood was *red*. This was strongly marked in the gills, which were found in the shape of a series of rows of

simple lamellæ, hanging from the internal surface of the mouth; thus proving the correctness of Vogt's observations from alcoholic specimens. At times the peduncle would become congested, and a deep rose blush was markedly distinct. The sexes were distinct. The writer believes the Brachiopods to be true articulates, having certain affinities with the crustacea, but properly belonging to the worms, coming nearest the tubicolous annelids. They may better be regarded as forming a comprehensive type, with general articulate features. Possibly they have affinities with the mollusks, through the homologies pointed out by Allman as existing between the Polyzoa and Tunicates. It is interesting to remember that Lingula, though one of the earliest animals created, has yet remained essentially the same through all geological ages to the present time.—*American Naturalist.*

Itacolumnite under the Microscope.—Professor A. M. Edwards read the following note on the subject at the New York Lyceum of Natural History, on the 23rd of May:—To those accustomed to the use of the microscope it is not a matter of surprise that persons who do not commonly employ that instrument in research should make very serious mistakes in interpreting what they think they see by means of it; the more especially when high powers of magnification are made use of. The delicacy of manipulation necessary to work with the microscope at all satisfactorily and the education of the eye required for the proper seeing by means of it are not generally understood, so that those who are not skilled microscopists are extremely liable to be led into error. What I am inclined to consider a case of this kind has lately been brought to my attention, and I am persuaded to make a note of it the more for the purpose of correcting a grave error in investigation, and one which is, strange to say, readily demonstrated to be an error. In 1867,* Dr. C. Wetherill published a well-written and seemingly-exhaustive paper, setting forth some "Experiments on Itacolumnite, with the explanation of its flexibility and its relation to the formation of the diamond." In attempting to elucidate the flexibility of this rock he has made use of the microscope, and, in fact, mainly draws his conclusions from the revelations which he supposes that instrument makes. Some specimens of Itacolumnite, varying in tint from almost pure white to a rusty red tint, and in texture from finely granular to coarse and distinctly laminated, having come into my hands, I have been enabled to examine into this point of structure and attendant flexibility, which I have done with some care. It is well known that Itacolumnite is the accompanient and often the matrix of the diamond, hence the interest which attaches to its peculiarities, as it would seem in some way to be connected with the occurrence or formation of that gem. Detecting dark-coloured grains in it, Dr. Wetherill considers them to be black diamonds, and doubtless he is correct in his supposition, but with this portion of his paper I do not desire to deal at the present time. It is with regard to the structure of the sandstone whereby it becomes flexible to the remarkable degree so evident when thin slabs are examined. The

* 'Amer. J[illegible]' vol. xliv.

stone is plainly laminated, and has clearly been thrown down beneath water, it being readily cleavable into more or less distinctly marked laminæ. Almost universally the flexibility is attributed to the presence of mica, but the brighter-coloured specimens which I have examined contain no mica, and yet possess the property of being readily bent to a very marked degree. Even in the red-tinted specimens there are large portions in which no mica is found. Dr. Wetherill says, that by examining the Itacolumnite by means of the microscope he has been enabled to ascertain that the "flexibility is due to *small and innumerable ball-and-socket joints* which exist throughout the mass of the stone very uniformly. Each joint permits a slight movement which is always greater in one direction." Now I must say that, though I have come to the investigation prepared with considerable faith, yet, after many careful examinations, I was never able to force my imagination to the extent of getting it to show me anything resembling ball-and-socket joints. The examination need not always be made of the opaque sandstone, but portions can be roughly crushed and mounted in Canada balsam so that light may be transmitted through them and the mode of their interlocking plainly made evident. The fact is that the rock is made up of small, broken, irregular masses of transparent sand which evidently have not been carried any great distance by water, as their sharp edges have not been at all abraded, but, on the contrary, remain, but they have evidently been broken off from a rock which had a conchoidal fracture, they being little plates of extremely irregular outline. Thus, when they settled in the liquid in which we can suppose them to be thrown down, they naturally, for the most part, distributed themselves with their greatest axes in the same direction, and hence the lamination and cleavage of the rock itself. We can readily understand that in such a rock, if the particles were not strongly held together, that they would possess a certain amount of motion one over the other, and this motion would be most marked in a direction at right angles to the lamination, which is the case. But, also, such a rock would not be elastic, only flexible, and gradually, after several times bending, be broken. Such is exactly the case with Itacolumnite. In fact any one possessing a microscope and a fragment of this rock, can readily verify my observations and demonstrate that Dr. Wetherill's proposed name of Articulite is inappropriate for Itacolumnite. In conclusion, I would mention that grains of the crushed rock when put up in Canada balsam become very beautiful objects for examination by means of the micro-polariscope, exhibiting a gorgeous display of colours when the interposing selenite film is used.

The Structure of Graptolites.—Mr. John Hopkinson, in an article published in the 'Geological Magazine,' says that the name *Dicranograptus* was proposed by Hall, in his 'Memoir on the Graptolites of the Quebec Group,'* for certain forms of Graptolites, some of which were then included in the genus *Didymograpsus*, and others in *Diplograpsus*. He, however, only considered these forms as constituting a sub-generic group of his genus *Climacograptus*, believing them to have

* P. 112.

a similar structure. Mr. Carruthers then described this group as a distinct genus, restricting it to the typical species *D. ramosus*, and its allies; and at the same time, from an examination of British specimens, he dissented from Hall's conclusion as to its structure, which he considered to be exactly similar, in its proximal portion, to *Diplograpsus*. He has already expressed the opinion that "in *D. ramosus*, the hydrothecæ appear to have the same structure as in *Climacograptus*," and he endeavours to show that, so far as can be judged from the imperfect state of preservation in which our British specimens are found, not only this species, but the entire genus, although frequently somewhat similar in the form of its hydrothecæ to *Diplograpsus*, is, nevertheless, more nearly allied to *Climacograptus*. This he does in the description of some species in the 'Geological Magazine' for August.

A New Photometer.—A photometer, invented by M. Nagant, is based upon the formation of a column of liquid, partially opaque, which may be drawn out until the length is such that the light from an illuminating body ceases to be visible through the liquid. The length of the column, which completely obscures the light, starting from the point where the column is thinnest, gives a measure of the intensity of the light under examination.

Palæocoryne, a New Fossil Hydrozoon.—This, which is a genus of *Tubularian Hydrozoa*, from the Carboniferous formation, is fully described by Dr. P. M. Duncan, F.R.S., and H. M. Jenkins, F.G.S., in the 'Philosophical Transactions' for 1869. The remarkable little organism which forms the subject of this paper was obtained from the lower shales of the Carboniferous Limestone series of Ayrshire and Lanarkshire, so rich in fossil Brachiopoda, Polyzoa, Crinoidea, and Madreporaria; and was found attached to the margins of the polyzoarium of *Fenestellæ*, and also in a detached and more or less fragmentary condition amongst the small pieces of broken Polyzoa and Crinoid stems which compose the fossiliferous layers of the shales. The base of *Palæocoryne* was expanded, giving rise to a short robust and cylindrical stem fluted and punctated on its surface, and surmounted by the body of the polypite from the upper margin of which radiate a single whorl of long and slender tentacles. On the upper surface of the body, a crateriform process, with an opening on its apex, indicates the position of the mouth. Its external investment appears to have been calcareous, covering the whole of the hydrozoon, except at the opening for the mouth and the terminations of the tentacles, which had probably ciliated ends projecting beyond the periderm or polypary. This is an almost solitary instance of a hydrozoon having a hard periderm, save the recent genus *Bimeria*, discovered on the west coast of Ireland by Dr. T. Strethill Wright. The Zoological position of the fossil is amongst the Hydrozoa in the order Tubularidæ, and near the Eudendridæ. Two species are described and figured by the authors, *Palæocoryne Scoticum* and *P. radiatum*.

Regeneration of Bone by Periosteum.—M. Ollier's recently reported cases seem to prove the fact of regeneration of the bones through the

periosteum. The two cases died, but at sufficient time after the operation to show the advantages of the remedy. Both were cases of resection of the elbow-joint. One died eighteen months and the other a year after the operation.

Death of Albrecht Von Graefe.—The 'Lancet' records the death of this distinguished man. Besides some researches which may be considered to give him standing as a microscopist, he had done much to advance our knowledge of eye structure. Indeed he may safely be said to have been the first ophthalmic surgeon in the world.

Death of Minute Organisms.—A paper which is of some interest, though containing many facts which are now regarded as absolutely proven by the advanced microscopist, is published in the 'Lancet,' August 6th. Mr. John Dougall, M.B., is the author, and he states that the number of genera of organisms operated on by him is undoubtedly small; still they represent a number of facts, forming a quota of the truth. The entomostraca, though not microscopic, are yet very small in size, being about $\frac{1}{50}$th of an inch in their short diameter. These were added to the list from their being easily procured, and also for the purpose of comparing their susceptibility to the action of the poisons with that of the other organisms. A considerable number of them happened to be pregnant females, having their purple ova-sacs attached externally at the free abdominal extremity, which were soon shed or aborted under the influence of the poison. The species was exclusively *Cyclops quadricornis* (Müller), taken in fresh water. The infusoria were obtained from water in which hay had been steeped for two weeks. The spermatozoa were human, and regarding these it may be stated that it has been ascertained by Kölliker, quoted in the 'Cyclopædia of Natural History,' "that the over-dilution of the semen with water causes the filaments to form loops, and their motions to cease, and that they are revivified by the addition of such substances as albumen, syrup, &c." My observations on the spermatozoa have been such as to make me accept the first part of this statement with some qualification. Probably were the semen very largely diluted with water, the phenomena would be such as described by Kölliker; but it has been found that two parts of water added to one part of the spermatic fluid has only the effect of curtailing the vivacity of the filaments, considerable motion being still quite perceptible. In regard to the latter part of Kölliker's statement—namely, "that the filaments can be revivified by the addition of albumen, syrup, &c., when rendered motionless in dilute aqueous solutions," it was understood that he meant *solutions which are very dilute;* and as most of the substances which have been stated as causing the death of the filaments may be considered as *very dilute*, it was assumed that the cessation of motion might be the result of such great dilution, and consequently that, according to Kölliker, the addition of syrup or albumen would prove whether the filaments were merely motionless or actually dead. In each case, then, after it was seen that they had ceased to move, an attempt at resuscitation was made by adding a little syrup or albumen, and it may be stated that in no case was motion renewed. A uniform method in bringing the various poisonous menstrua in contact with

the organisms was strictly observed. As far as practicable, a part of the poisonous solution, equal to the amount of fluid on the slide, was in every case added. It was found that when two drops of the former were added to one drop of the latter, the effect was much more marked than when equal parts were brought in contact. This was of course to be expected, two drops containing double the quantity of poison in one drop, though the degree of dilution was the same. It will be observed in the subjoined Table that the spermatic filaments are capable of existing in a very concentrated solution of common salt ($\frac{1}{10}$th). In more dilute solutions, as $\frac{1}{100}$th to $\frac{1}{250}$th, their movements are greatly accelerated. I was not aware, when experimenting with this substance, that the same effects had been observed by Quatrefages, Newport, and others. It will be observed, also, that the various poisons were allowed to act on the organisms for a uniform period of time—*i. e.* the spermatic filaments were kept immersed for fifteen minutes in every case; so also were the entomostraca; and though the regularity of the data is disturbed by the infusoria having been allowed only two minutes' immersion, still the relative effects of the various solutions in a given time is shown: the object being to ascertain, in the case of each distinctive poison applied separately to the different genera of organisms, the exact quantity requisite to destroy them in a definite time.

TABLE SHOWING THE QUANTITY OF VARIOUS SUBSTANCES IN AQUEOUS SOLUTION REQUIRED TO KILL, IN A GIVEN TIME, SPERMATOZOA, INFUSORIA, AND ENTOMOSTRACA.

Substance.	Spermatozoa.	Infusoria.	Entomostraca.	Average.	Remarks.
Hydrochlorate of strychnia	1–30000	1–450	1–8000	1–12816·6	The movements of the infusoria greatly increased at first.
Hydrochlorate of arsenic	1–18000	1–8000	1–2000	1–9333·3	
Nitric acid	1–18000	1–1500	1–300	1–6600	
Hydrochloric acid ..	1–15000	1–2000	1–300	1–5766·6	
Sulphuric acid.. ..	1–15000	1–1500	1–500	1–5666·6	
Alcohol	1–12500	1–750	1–2000	1–5083·3	Infusoria when dying make small circles.
Bichloride of mercury	1–7000	1–6000	1–1500	1–4833·3	An entomostraca lived for 15 minutes in 1–2000, and was removed quite lively.
Nitrate of silver ..	1–6500	1–6000	1–2000	1–4833·3	
Acetic acid (*fort.*) ..	1–10000	1–500	1–50	1–3516·6	
Oxalic acid	1–7500	1–1500	1–1500	1–3500	
Chloride of zinc ..	1–7500	1–600	1–500	1–2866·6	
Picric acid	1–3700	1–450	1–2000	1–2050	
Tartrate of antimony	1–4000	1–450	1–500	1–1650	Infusoria when dying make short and sudden darts.
Hydrocyanic acid ..	1–5000	1–250	1–500	1–1583·3	
Carbolic acid	1–1000	1–750	1–1500	1–1080·3	
Camphor	1–2500	1–400	1–250	1–1050	
Tincture of iodine ..	1–500	1–400	1–500	1–466·6	Entomostraca struggle violently from the time they are put in the solution.
Solution of chloride of lime	1–500	1–300	1–250	1–350	
Common salt	1–10	1–50	1–100	1–53·3	Spermatozoa are rendered very lively by weaker solutions of common salt.
	Average. 1–7079·47 Time 15 m.	Average. 1–1676·3 Time 2 m.	Average. 1–1165·79 Time 15 m.		

A Testimonial to Professor Morris.—Professor John Morris, F.G.S., of University College, London, has received from his several admirers a testimonial and upwards of 600*l.* A meeting was convened on the 14th July, at the apartments of the Geological Society, Somerset House,

Sir Roderick Murchison, Bart., being in the chair, supported by the President of the Geological Society, J. Prestwich, Esq., F.R.S., the Treasurer and Secretary, and a numerous assemblage of Mr. Morris's friends. An address (the wording of which had been entrusted to Prof. Phillips, F.R.S., of Oxford), beautifully engrossed and illuminated on vellum, and framed in a suitable manner, was presented to Prof. John Morris by Sir R. Murchison, together with the proceeds of the subscription, and the Silurian chieftain made a most admirable speech, in which he was followed by Mr. Prestwich, both these gentlemen being among Prof. Morris's earliest and most attached friends. In returning thanks, Mr. Morris (who was most heartily cheered) made a most touching and eloquent speech, in which he referred to his early geological labours and travels, nearly thirty years ago, in the company of Murchison and Prestwich. Mr. Prestwich afterwards read a letter just received by Mr. Hearn, the Secretary, from the venerable Prof. Sedgwick, expressing his regret that the infirmity of years prevented his being present, but assuring Mr. Morris of the cordial esteem and regard which he entertained for him and for his scientific labours. After all, the honour was long overdue.

A curious Flesh Parasite has been figured in the Lancet by Dr. Tilbury Fox. It is thought, he says, to possess jaws and to bite freely; and certain of the wounds seen on the skin are regarded as having been produced by the bites of the pediculus. This is all a mistake. Prof. Schjödte has clearly shown that the pediculus is furnished with a peculiar sucking apparatus. The mouth is furnished with a labium, capable of being retracted into the upper part of the head. This lip is first inserted into a sweat-pore, and is then protruded. A row of hooks then hold to the parts around, and two pairs of setæ are next protruded and applied together so as to form a tube. When the pediculus is sucking, soon a red speck is seen at the top of the head, which exhibits dilation and contraction; and this red coloration is traced presently into and along the œsophagus and the intestines, which latter are seen to be in lively peristaltic action. The effect of the attack of the pediculus is to cause a little escape of blood into the follicle; and it appears as a minute and, at first, bright red speck, the size of a couple of pin points—not *raised*, not itchy, and not removable by pressure. Occasionally some swelling takes place; but this quickly subsides.

NOTES AND MEMORANDA.

Continuously Adjustable Diaphragm.—Mr. J. Zentmayer, of Philadelphia, U.S., some time since devised and constructed a diaphragm, of which the accompanying woodcuts are views in two conditions of adjustment.

Two cylinders or rollers of equal size and parallel axes are so mounted as to be revolved in contact by fine gearing at one end. Similar conical grooves are arranged in the surfaces of these in such a manner that they produce together a circular opening, which will of course vary its size according to the position of the cylinders in their revolution. There is theoretically an objection to a diaphragm of this construction, from the fact that its opening will not be always in the same plane. With the largest opening, the smallest section at right angles to the line of sight will be nearest to the plane passing through the axes of the cylinders, and with the smallest opening it will be farthest from this plane. This difference is, however, very small in fact, and might in certain cases become advantageous.

CORRESPONDENCE.

Dr. Pigott and Mr. Wenham.

To the Editor of the 'Monthly Microscopical Journal.'

Ashdown Cottage, Forest Row, Sussex,
July 21, 1870.

Dear Sir,—I am sorry to find, from Mr. Wenham's letter in your last number (July), that I helped to mislead him concerning Dr. Pigott's experiments, by speaking of the microscope being *converted* into a telescope. I did not mean *used* as a telescope, but as Dr. Pigott's method is explained in the 'Proceedings of the Royal Society,' to which you have referred, I need not now say more on this subject.

I hope Mr. Wenham will before long have an opportunity of fully testing Dr. Pigott's statements concerning aberration, as if he, Mr. Wenham, is once convinced that any mischievous amount of error exists in our best objectives, his remarkable practical skill will be immediately directed to the best method of cure. He is of all men the most likely to be successful in this difficult matter.

I remain, dear Sir,

Yours faithfully,

Henry J. Slack.

PROCEEDINGS OF SOCIETIES.*

Quekett Microscopical Club.†

The annual meeting was held at University College, July 22nd, 1870, P. Le Neve Foster, Esq., M.A., President, in the chair. The fifth annual report, read by the Secretary, warmly congratulated the members upon the continued success and usefulness of the club, whose members now exceeded 500, 57 having joined during the year. The cabinets contained upwards of 1300 slides, and important additions had been made to the library. Especial reference was made to the courtesy and kindness of the authorities of University College, by whose permission the meetings of the club were held in that building.

The Treasurer's report showed a balance in hand of 26*l.* 9*s.*, and the report of the Librarian detailed the additions made in his depart-

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

† Report furnished by Mr. R. T. Lewis.

ment during the year. On the motion of the President, seconded by Dr. R. Braithwaite, the reports were unanimously received and adopted.

The President then delivered an address to the members, in which, after reviewing the past history of the Club, and pointing out those elements which had mainly contributed to its success, he offered some valuable counsel as to the future, urging the desirability of each member choosing some single object for careful study rather than wandering aimlessly over a wide field, or searching for mere tests of instrumental power, and suggesting the valuable aid which might be rendered to science by the active co-operation of several individuals in following up some selected object. Closer acquaintance with the optical principles of the instrument with which they worked, and the reduction of the vast mass of literature at command, were also recommended, and the importance of drawing and mathematics as accessories to microscopical study was incidentally alluded to. The address was concluded by hearty expressions of desire for the future welfare and usefulness of the Club, and the President resumed his seat amidst loud and prolonged applause. A vote of thanks to the President for his services during his year of office, and for his admirable address, was moved by Mr. M. C. Cooke, seconded by Dr. Braithwaite, and carried unanimously. Mr. S. J. McIntire then read an interesting paper "On the Pencil Tail"—*Polyxenus Lagurus*—illustrated by drawings, and by the exhibition of both adult and recently-hatched living specimens. A vote of thanks to Mr. McIntire was carried *nem. dis.* The election of officers for the ensuing year was then proceeded with, and upon the report of the scrutineers being handed in to the President, the following gentlemen were declared by him to be duly elected:—As President, Dr. Lionel S. Beale, F.R.S.; as Vice-Presidents, Dr. Braithwaite, Mr. A. E. Durham, Mr. P. Le Neve Foster, and Mr. Henry Lee; as Members of Committee, Messrs. Allbon, Burr, Bywater, and C. F. White; as Hon. Sec., Mr. T. C. White; as Treasurer, Mr. Hardwicke; and as Hon. Sec. for Foreign Correspondence, Mr. M. C. Cooke. Mr. Foster having briefly introduced his successor in office, vacated the chair, which was taken, amidst great cheering, by Dr. Lionel S. Beale, F.R.S., who expressed the pleasure which it afforded him to come amongst the members of the Club, and his sense of the honour conferred upon him by his election as President. An unusual number of presents to the Club were announced by the Secretary, including a handsome tank-microscope with 4-inch objective from Mr. Ross, for which special thanks were returned.

Three new members were elected, seven gentlemen were proposed for membership, and the proceedings terminated with a conversazione, at which a variety of interesting objects were exhibited.

Brighton and Sussex Natural History Society.

Microscopical Section.

(*Continued from p.* 120, *No. XX.*)

The discovery of the alternation of generations was due to careful study, and the knowledge of the fact of the Polyps of our shores having other existences as free-swimming Medusæ considerably modified our previous ideas respecting them. The well-known sections of insects, by Dr. Hallifax, show the viscera and nervous system undisturbed, and —better still—the advantage to others, as well as ourselves, of well-directed work. As a concluding illustration, I may mention that the stupendous tale of past life, read by the microscope in our chalk cliffs, would have been incomplete without that systematic comparison with the bottom of the present ocean, which has proved the persistence through ages, not only of the laws of Nature, but of the forms of life. I have not chosen pathological examples, notwithstanding the pre-eminent importance of microscopic examination in pathology, as its study is scarcely within our province. I hope, however, that I have sufficiently supported my opening statement of the advantage of systematic work in place of such desultory observations as are comparatively worthless. Our interest must increase with well-directed study, and numberless facts which we at first pass by, unnoticed, may eventually teach lessons of lasting use. The prism had shown the spectrum to thousands before Newton's intellect gave its interpretation; and, although the comparison may seem far-fetched, there are subjects within range of the microscope which, by the exercise of thought and reason, may still further demonstrate the connection of the infinitely great and the infinitely little, and assist us to know more of the life which it has already taught begins in a germ,—apparently the same, whether it results in the highest or lowest of organized beings. We can all make some advance to such knowledge by the systematic study of recent things, which I have endeavoured to recommend, even by tedious repetition; and I trust that by our work we may justify the formation of this Section of our Society.

The meeting then resolved itself into a conversazione, at which the following gentlemen exhibited microscopic objects, under a goodly array of microscopes by the best London makers:—Mr. J. Dennant exhibited sections of fossil teeth from the Coal-measures; Dr. Hallifax showed some of his sections of insects, in which the internal parts were displayed *in situ*, one of the most striking being the lady-bird. Mr. Cooper exhibited deep-sea soundings from different localities, and Foraminiferæ from Hastings, the Mediterranean, and Australia. Mr. Sewell exhibited injected preparations of Dr. Thudicum's rabbit, an animal possessing a world-wide reputation from being fed at times on muscle containing *Trichina spiralis*; the presence of entozoa was traced in all parts of the voluntary muscle, but nowhere else. Mr. Aylen showed a number of entomological preparations. Dr. Addison exhibited blood, as acted upon directly by various agents, such as diluted sherry, &c. The effect produced was an alteration in the appearance of the blood discs, and,

as some expressed it, the formation of tails. This was considered by no means the least interesting part of the evening's display. Mr. Peake exhibited the *pygidium* of the lace-wing fly, *Chrysopa perla*, discovered by himself. The existence of this peculiar structure has long been known in the flea, but has not been pointed out in any other insect. Dr. Kebbell exhibited with a Nachet's prism the rasping teeth situated on the disc of the proboscis of the blow-fly. Mr. Smith showed fruit of Hepaticæ and Epicarpal Stomata of moss, *funaria*. These are only found on the fruit of mosses, and never on the leaves. Mr. Hennah showed plant circulation in the hairs of the *Tradescantia*, spider-wort, and remarked that every microscopist should possess a root of this plant in his garden; pollen showing the production of the pollen-tubes; Desmideæ, mounted five years since in the water in which gathered, with the endochrome perfectly preserved; and Caprellæ, from Black Rock, Kemp Town, &c. Mr. Wonfor exhibited a slide of Diatom, mounted by Müller, of Holstein, on which, in the space of a quarter of an inch, 408 separate silicious skeletons of plants were arranged in symmetrical rows. This slide, which was a marvel of skill in microscopic mounting, arrangement, and perfection of specimens, was kindly lent for the occasion by Mr. T. Curteis, of Holborn. It was to the student of this department not simply a wonderful slide, but a cabinet of objects; in fact, a printed catalogue lay by the side of the microscope for reference to any particular valve; live caterpillars of the emperor moth; very gorgeous crystals of *hematoxylyn*, the colouring matter obtained from logwood; artificial alizarine, prepared from an oil of coal gas. The discovery of this substance is regarded as a great triumph in chemistry, as it resembles the red dye obtained from madder; polyzoa, and other interesting objects.

In addition to these microscopic objects, Dr. Hallifax exhibited a number of beautiful micro-photographs of his own taking; and Mr. J. Howell, pebbles picked up on the beach, showing encrinites, pentacrinites, bryozoa, cidaris spines, &c.

June 9th. Ordinary meeting.—Mr. Sewell, Vice-President, in the chair.

A paper "On Diptera and their Wings," by Mr. Peake, was read in the absence of that gentleman by Mr. Wonfor, Hon. Secretary.

While wings are common to the whole order of insects, the Diptera consist entirely of two-winged flies, which, instead of a second or hinder pair, have little thread-like bodies terminated by knobs and called *halteres*, originally considered balancers, supposed now by some to be organs of hearing, and by others *olfactory* organs. From many points of resemblance, he thought they were analogous to the hind wings of other insects, and that, at present, their special use had not been ascertained. Besides these halteres, they had also winglets (*alulæ*), which were thought to be only appendages to the fore-wings. Among the Diptera three classes of fliers were found, differing in the form of their bodies and shape of their wings; first, the slender flies, such as the gnats, having long bodies, narrow wings, and long legs, but without winglets; secondly, those whose bodies, though slender, were more weighty, as the Asilidæ, having larger bodies, shorter legs, and very

minute winglets; lastly, those like the house-fly, with short, thick, and often very heavy bodies, furnished with proportionate wings, shorter legs, and conspicuous winglets. From these circumstances it might be inferred that the long legs of the light-bodied flies acted as rudders, while the winglets helped the wings in flying. The wings consisted of two laminæ united by veins or nervures, and upon their arrangement and the form of the antennæ, as seen in the great groups Nemocera and Brachycera, the distinguishing characters of the Diptera are founded. The several parts of the wings and their nerves, and their differences as seen in the two above-mentioned groups, was next pointed out, and the paper illustrated by very beautiful drawings and microscopic preparations of wings.

June 23rd. Microscopical Section.—Mr. Glaisyer, Vice-President, in the chair.

Subject for the evening, *Infusoria,* which Mr. Wonfor introduced by a few remarks.

As all were aware, he said, if any vegetable or animal substance was placed in water, in a few days the water would be found full of minute organisms, to which the name of *infusoria*, or infusion animalcules, was at first given. Many of these had by later observers been placed in the vegetable kingdom; while others, at first named as distinct species, had been proved to be the early stages of higher animals, and others were classed among another group of animals. The class *Infusoria* was therefore much more limited than at one time supposed to be, and increased knowledge might prove that many more were only the early stages of other types of life. Mr. Wonfor then proceeded to point out the nature of their substance, mode of development, increase, and propagation. So widely were they distributed that scarcely anywhere could water be found which did not contain some type. Many would only live in fresh, others in salt or brackish water; while some were only found in water containing decomposing vegetable or animal substances. Hence, water contaminated by sewage matter always shows certain types. Many were found only in particular infusions, while some were common to several. Their appearance under certain conditions had led to theories respecting spontaneous generation, a much debated and debatable point; but as the atmosphere seemed, according to Tyndall and others, to be full of germs, their sudden appearance in favourable situations was not surprising. The water in which flowers were kept would yield some kinds; in fact, he had an abundant supply of one kind from water in which mignionette had been only three days. Water in bird fountains and water bottles, unless looked after, would be sure to contain *infusoria*.

The rest of the evening was spent in the examination of the different forms of *infusoria* brought for exhibition.

It was announced that the subject for the next meeting, July 28th, would be the "Eggs of Articulata."

July 14th. Ordinary meeting of the Society.—Mr. Hennah, M.R.M.S., President, in the chair.

Mr. Wonfor reported the receipt of one of Müller's Diatom Type-

slides from Mr. Hollis for the Society's cabinet, and of books from Miss Barnard and Mr. Nourse for the library.

Mr. Hennah gave an account of the Dredging Excursion of the previous Saturday, and spoke in terms of praise of the "towing net" as being almost invaluable in all expeditions.

Mr. Wonfor read a paper "On the Annual Excursion," in which the chief incidents of the day and the places seen and visited were described.

Mr. J. Robertson read a paper "On Sussex Centenarians, with remarks on Thomas Guerin, said to be in his 105th year."

July 28th. Microscopical Section. Mr. T. H. Hennah, F.R.M.S., President, in the chair.—Mr. T. W. Wonfor read a paper "On the Eggs of Articulata."

While the eggs of all animals differed in their appearance and markings, the general characters remained the same, *viz.* the germ vesicle, the yolk substance, and the vesicular envelope; the chemical constituents of these substances were albumen, fatty matter, and a substance not precipitated by water, the whole being enclosed in a shell membrane or *chorion*, in some cases provided with a lid or *operculum*, to facilitate the escape of the larva. This lid was very palpable in some eggs, but neither so evident nor its use so apparent in others, seeing the larvæ ate their way through the shell itself. At the apex of the eggs was a point called the *micropyle*, from which it was asserted the larva *always* emerged, and with which its mouth was connected, so that it might receive moisture from without; the examination of a large number of eggs of different species had convinced him that so far from the mouth of the larva being situated at the micropyle or that the creature escaped by it, being the fact, it was quite the exception in a vast majority of cases. The colour of eggs seemed to depend upon the colour of the yolk globules, and changed as the embryo advanced, eventually becoming dark brown or black. In some eggs the changes could be watched, but in others the chorion, consisting of three layers, was so thick as to prevent examination, except by very delicate dissections. The females, as a rule, deposited the eggs; but many examples occurred in which they were retained by her and deposited as larvæ. This was well seen in the *crustaceæ*, many of which hatched them, either in external ovaries or in a space between the body of the parent and the posterior part of the shell; in the black beetle (*Blatta orientalis*), which retained the larvæ in egg boxes until they were ready to emerge; in the blow-fly, in which the eggs were hatched within the body and deposited as maggots; and in the *coccus*, which converted her body into a shield for the protection of the eggs. Some creatures, by means of special apparatus, placed their eggs in the food of the larvæ; some, like the ichneumons, within the bodies of other creatures; and while some made no other provision than that of placing the eggs on suitable food, others constructed cells, suspended them in cocoons, covered them with varnish, rolled them in pellets, or glued them to the hair or feathers of animals. As objects for the microscope, apart from their physiological interest, they were among the most beautiful things in nature, presenting an

almost infinite diversity of form, colour, and markings. Perhaps no other class of objects from the animal kingdom afforded so great an absence of uniformity and so much beauty. Among some of the most striking were those of the earth mite, which laid discoid eggs on stones on the Downs; one of the diptera, which deposited winged, honeycombed eggs; or the lace-winged fly, which attached greenish-white elliptical eggs by long stalks to the leaves of plants infested by *Aphides*, on which the larvæ fed. That afternoon he had discovered that they also fed on the eggs of other creatures, a fact, as far as he was aware, neither noticed nor recorded. The eggs of the Puss moth looked like minute oranges; the cabbage butterfly laid a primrose-coloured egg, somewhat resembling a lobster trap; the common blue butterfly's eggs resembled exquisite ivory carvings, but some of the most striking were found among the bird parasites. Those of the Bohemian pheasant resembled some of the polycystina, those of the ground hornbill were so much like polyzoa that they might be mistaken for sea mats. The egg of the parasite of the mallee bird of Australia resembled the seed-vessel of the cornflower, while that of the Indian black-winged peacock might well be mistaken for an exquisite flower.

Those from which the larvæ had escaped made good objects; in some cases it was necessary to perforate and extract the contents to prevent their shrinking; others, after steeping in benzole, might be mounted as dry objects.

The rest of the evening was spent in examining a large collection of eggs, &c., provided by Mr. Wonfor, and in viewing some beautiful photographs of the eggs of bird parasites.

It was announced that the next meeting would be "an evening on mounting objects," when different gentlemen would give practical instruction in some of the different methods employed.

August 11th. The President, Mr. T. H. Hennah, in the chair.—Mr. W. Olding and Mr. J. P. Smith were elected members. Mr. Dennant reported on the last Field Excursion to Steyning, and Dr. Hallifax read a paper "On the Vertebrate and Invertebrate Eye compared." He commenced by saying that homology of structure with diversity of purpose exists throughout each of the great divisions of the animal kingdom.

The fore-extremity—with the same components—is the arm in man, the leg in the race-horse, the wing in the bird, the fin in the fish, and the paddle in the seal, all different expressions of uniformity of plan, but instead of *homology*, *analogy* exists between the vertebrates and invertebrates. Dr. Hallifax instanced the wing of the fly and bird as having the same function, but the former deriving its structure from its external, the latter from its internal skeleton.

The vertebrate eye, in like manner, presents the same homology throughout, and he chose the human eye as his example, as, in addition to our knowledge of its anatomy, we have personal knowledge of its functions. Aided by a large diagram, he fully explained its structure, mentioning that the retina—only about the $\frac{1}{120}$th of an inch in thickness—has five layers, and while the other parts of the eye fulfil

a mechanical or physical office, its office is vital or mental. He thought it necessary to dwell longer on the compound eye of the invertebrates, as our knowledge of its structure is still imperfect. Little advance had been made for 200 years, until the German physiologists lately showed that the eyes of insects and crustaceans—with a common function—present many analogies with those of vertebrates.

Dr. Hallifax exhibited diagrams of the eye of the dragon-fly, and pointed out the bulbed ends of the nerve fibres, which are not, as has been supposed, crystalline lenses, but true representatives of the rods and cones of the baccillary layer of the retina—the percipient element of the eye.

These bulbed nerves are each covered with pigment, except that where they come in contact with the lenses, there are apertures in analogy with the vertebrate pupil.

The lenses are the outer hexagonal facets, each a double convex lens of itself, the inner surface the most convex, and placed in contact with a bulbed nerve—each with its independent nerve forming a distinct eye. The facets in the compound eyes of insects can be numbered by hundreds; but as with the human eyes the two images are perceived as one, so the mental perception of the fly recognizes but a single object.

Dr. Hallifax called attention to the absence, in the eyes of insects, of the crystalline lens and vitreous humour of the vertebrates, although the essential lens and cones are represented.

The numerous facets of the eyes of insects allow, from their position, of extreme range of vision, and render unnecessary the powerful muscles of the vertebrate eye.

Dr. Hallifax considered the eyes of insects most fitly placed and formed, with reference to their organization generally, and said that we should always consider organs with such reference to general structure.

He then exhibited, under the microscope, beautiful sections of the eyes of insects, in demonstration of his subject.

In the discussion which followed, Dr. Hallifax was asked his opinion of the structure of the single eyes placed between the compound eyes of flies, and on spiders, as they appear to present a much closer approach to the vertebrate type than is allowed to the compound eyes. Dr. Hallifax explained that he accepted the authority of those who consider the spherical portion at the back of such single eyes as an enlargement of the cornea, and not a vitreous humour; he did not, however, extend his explanation to the arrangement of lenses and iris shown in one of his own sections of a spider's eye, and which seems almost identical with that of the human eye.

After a vote of thanks was passed to Dr. Hallifax, it was announced that the next Field Excursion would be to Lewes, on Saturday, September 3rd; that the Microscopical Section would meet on August 25th—subject, Mounting Objects; and that the meeting of the Society on September 8th would be the Special Annual Meeting, for the election of officers, &c.

American Academy of Natural Sciences.

Exhibition of the Biological and Microscopical Section.

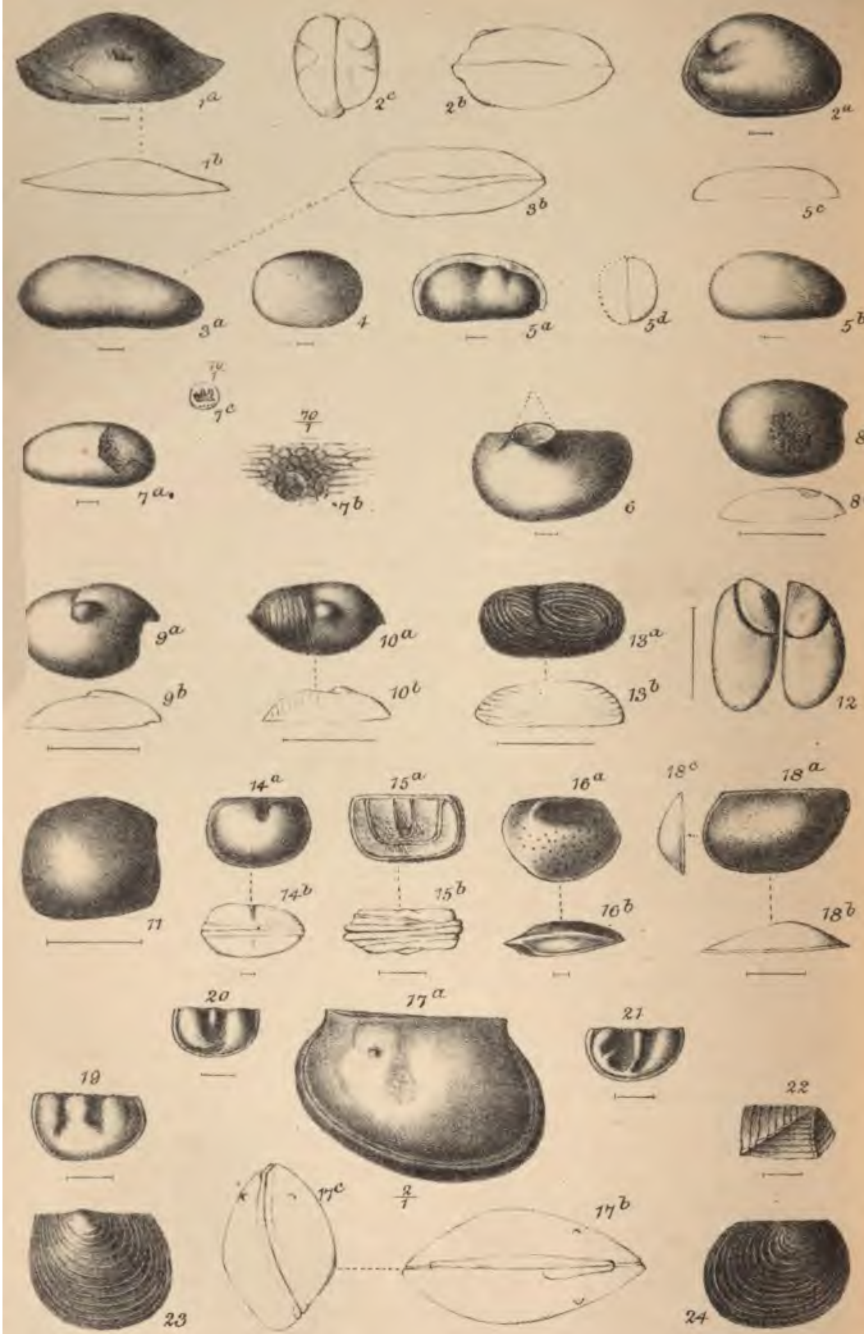
The Biological and Microscopical Section of the Academy of Natural Sciences, whose First Annual Conversazione was so highly successful and gratifying to all concerned, has established a still further claim upon the medical men of Philadelphia to promote its objects, by entertaining their guests, the members of the State Medical Society, at its late meeting in this city, with an exhibition of microscopes and microscopic specimens, at the Hall of the College of Physicians, on Friday evening, the 10th of June. As this reception was especially intended for professional men, the class of objects displayed was quite different from the majority of those shown at the conversazione, including many illustrations in pathological anatomy and histology, of course chiefly interesting to physicians; and no effort being made to illustrate the history of the microscopes, only about two-thirds as many instruments, numbering about forty-five in all, each, however, in efficient working condition, were collected from the members and arranged upon the tables. Among the many beautiful and instructive specimens displayed, the Director of the Section, Dr. S. Weir Mitchell, had on exhibition some very elegant preparations of teeth, bone, &c., mounted by Prof. Christopher Johnson, of Baltimore, and also a fine collection of blood-corpuscles from different animals. Dr. William Pepper, Vice-Director, further illustrated the subject by a series of blood-crystals of remarkable beauty. The wonderful amœboid movement, lately so famous in connection with both Cohnheim's discovery of the origin of pus and Huxley's great lecture "On Protoplasm," was well shown by Dr. J. G. Richardson, the Secretary, with a power of 1300 diameters; while the Corresponding Secretary, Prof. McQuillen, illustrated various departments of dental anatomy and physiology by magnificent sections of teeth of man and animals (showing among other points the inter-globular spaces), and of bone exhibiting the lacunæ and canaliculi. He likewise displayed very interesting specimens of human muscle infested with *Trichina spiralis*, from fatal cases of Trichiniasis; also the infecting swine's flesh. Prof. James Tyson had on exhibition a complete series of urinary deposits, which attracted much attention, and were among the most instructive objects presented. Dr. Wm. F. Norris contributed some exquisite specimens of nerves in the cornea, and capillaries showing their parietal nuclei—respectively gold and silver stainings—by the methods of Cohnheim and Recklinghausen. Some very large sections of brain, kidney, &c., were displayed by Dr. W. W. Keen, who also showed sundry fine illustrations of nerve structure. Dr. W. B. Corbitt exhibited a very valuable series of specimens of various malignant and other tumours, collected in Germany, and many of them classified under the supervision of the great pathologist, Prof. Rokitansky.

Mr. Walmsley (of the well-known firm of opticians, J. W. Queen and Co.) and Mr. T. W. Starr each contributed a number of the beautifully-mounted preparations for which they are so justly celebrated.

Mr. Zentmayer displayed, among other instruments, one of his splendid binocular microscopes, which was greatly admired. Prof. J. A. Meigs exhibited, with others, an injected specimen of the gall-bladder, made by Dr. P. Goddard many years ago.

Prof. B. H. Rand arranged upon a side table his spectroscopic apparatus, and from time to time demonstrated to eager spectators the spectra of different metals, and explained this new and exceedingly delicate method of analysis to the satisfaction of all.*

* Report supplied by J. G. Richardson, M.D.



Geo. West, del. et lith. W. West imp.

Palæozoic Entomostraca.

THE

MONTHLY MICROSCOPICAL JOURNAL.

OCTOBER 1, 1870.

I.—*The Patterns of Artificial Diatoms.*

By Henry J. Slack, F.G.S., Sec. R.M.S.

(*Read before the* Royal Microscopical Society.)

I hoped by this time to have laid before the Society the results of a series of examinations of a number of species of Pinnulariæ, showing that these diatoms have been much misunderstood by microscopists, who have accepted their separation from the Naviculæ on the ground of their so-called *costæ* not being resolvable into dots, or beads. I was led some time since, by carefully viewing the row of Pinnulariæ in Möller's "type slide" with Beck's $\frac{1}{20}$th, to doubt the correctness of the usually-received views, and on repeating the investigation with Powell and Lealand's new immersion $\frac{1}{8}$th it became evident that the costæ of most, if not all, were less simple in structure than had been imagined, and could be resolved into more or less complex beaded forms. An application to Möller, through Mr. Baker, for a set of the Pinnulariæ mounted dry has not yet proved successful, probably owing to the disturbance of rational pursuits consequent upon the war, and though Mr. Norman supplied some excellent slides, he had not in his collection all the species required.

A general survey of the diatoms in Möller's "type slide" enables a gradation to be traced between beads of large size, widely separated under moderate powers, and similar beads closer and closer together, until real or closely-approximate contact is obtained. Another gradation may likewise be traced in size, from beads which are conspicuous like little marbles with a magnification of a few hundred diameters, to others that appear extremely minute when high powers with deep eye-pieces are used for their examination. The question naturally arises, Where does the beading stop? Does it stop at all?

A living object immersed in, or supplied with, water having silica, or a silicate in solution, and causing, during its life processes, a deposition of silica to strengthen its own tissue, must evidently obtain it by the method of chemical precipitation, and probably, in the case of the diatom, by decomposing a soluble silicate of soda or potash. If all diatoms were constructed upon the same principle, and by the same process of chemical precipitation, a uniform plan

would probably be found in their silicious structure, and this expectation, rather than any anxiety to see dots that others had not discerned, interested me in the Pinnularian inquiry.

One portion of such an investigation obviously relates to the patterns which can be obtained by purely chemical processes, and without the intervention of any kind of life.

In this paper those forms only which occur in Schultze's artificial diatoms will be alluded to, and it may be as well to premise that although no diatom is likely to obtain its silica exactly in that way—that is by means of a silicic fluoride—Schultze's formations may still throw out much light upon the whole subject.

To obtain the artificial diatoms, powdered glass and fluor spar are acted upon by sulphuric acid, or hydric sulphate as the new school of chemistry prefer to call it. If no heat is used, the silicic fluoride gas rises slowly, and if allowed to impinge against threads of cotton moistened with water, a decomposition takes place, "one third of the silicon unites with the oxygen of the water, and is thrown down."* If the gas is passed through water the precipitation takes place in white flakes quickly choking up a small tube. When the moist cotton threads are used, and the process goes on slowly, a great quantity of irregular sausage-like tubes are formed, and when these are well washed, to get rid of the hydric fluo-silicate, and crushed under a covering slide to obtain surfaces flat enough for convenient examination, the diatom patterns will be seen with various powers from a $\frac{2}{3}$rds to the highest that are made.

The Society will excuse in these last remarks my repeating some matters that are now old, and which has been done to make the subject more intelligible to that, I fear, very numerous class of microscopists who have not paid to Schultze's artificial diatoms the attention they deserve.

The mode of their production just described will suffice to indicate that the circumstances determining the patterns in which the silica is precipitated will depend upon conditions presenting only slight differences. If the whole process is carried on in a room of ordinary temperature, say about 60° or 70°, and completed in a few hours, changes resulting from more or less heat will have little effect. The rate at which the silicic gas is evolved, the quantity of moisture it meets on the cotton threads, and such facts as the closer or looser arrangement of those threads, would seem to be the chief causes of any modification that occurs. Thus we may safely ascribe all the patterns we obtain to very slight chemical and physical conditions of decomposition and deposition, and yet what varieties of arrangement and size of dots or beads we obtain. The following is a description of some of the varieties in a single slide. The sizes are *apparent* with a magnification of about 700.

* Barff's 'Chemistry.'

No. 1. Pattern regular, quincuncial, beads appearing about 1—4″ diameter, interspaces double that of the beads' diameters.

No. 2. Pattern regular, quincuncial, beads about half the size, and interspaces about half those of No. 1.

No. 3. Beads appearing about 1—7″ in diameter and touching. This pattern readily gives the hexagonal aspect obtainable with many diatoms.

No. 4. Regular rectangular pattern, beads appearing about 1—20″ in diameter.

No. 5. Similar beads to 4, grouped very irregularly with wide irregular interspaces, in which no beads could be discerned.

No. 6. Similar beads in rows wider apart in one direction than in another, the interspaces being unequal in rectangular directions and many times the diameter of the beads.

No. 7. Rather larger beads irregularly arranged, some lines straight, and others curved.

No. 8. Large beads appearing from 1—8″ to 1—7″ in diameter surrounded by beads a quarter their size.

No. 9. Large composite bead, as if composed of three adhering and compressed beads, surrounded with smaller beads irregularly arranged, and the interspaces filled with innumerable minute beads.

No. 10. Large bead, surrounded by concentric circles of smaller beads.

No. 11. Small beads in concentric rows, like a fragment of a delicate circular diatom.

No. 12. Beading so fine and close as not to admit of distinct separation with mag. 1000.

No. 13. Beading only indicated, and distinguished from clear-looking spaces somewhat as a surface of dead-gold is distinguished from a burnished one.

No. 14. A form approximating to some of the polycistina, but with large beads as well as holes.

The above by no means represent all the varieties noticeable in a single slide. In one case large beads appeared built up of smaller ones, but the optical appearances were too puzzling to say exactly what the true form was. It seems probable that the clear spaces only differ from the beaded ones in the fact that the silex, though precipitated in spherules, has formed a pattern too minute to be discerned.

An examination of the artificial diatoms shows that purely chemical and physical considerations will account for the varieties of pattern we notice in natural diatoms, and their living structure appears only to provide the conditions under which the silicious precipitation takes place, according to the ordinary laws of chemical action and molecular coalescence.

II.—*On Ancient Water-fleas of the Ostracodous and Phyllopodous Tribes (Bivalved Entomostraca).*

By Professor T. Rupert Jones, F.G.S.

Plate LXI.

Part I.—The Leperditiadæ.

The "water-fleas" are a mixed group of minute Crustaceans, some in fresh and others in salt water, which received their common English name from their jerky movements, and their continental name of "Entomostraca," or "shell-insects," from their somewhat insect-like appearance and their shell-like cases. The "water-fleas" proper, however, are the *Daphniadæ* and their near allies, mostly of fresh-water habits. These have for the most part a very delicate, thin, horny envelope, or carapace, bent down on either side over, and enclosing, the body, but not divided and jointed along the back. Of these there are no well-known fossil representatives; but certain remains of such soft-shelled water-fleas are recorded by M. Mahony, in the 'Geological Magazine,' No. 63, p. 392, as occurring in a Post-tertiary clay in Renfrewshire; and some possibly Daphnioid Entomostracans are noticed in the same Magazine, No. 71, p. 219, as coating a layer of coal-shale from South Wales. Some old fossils have been referred to *Daphnia* on a mistaken notion of similarity of form.

Other water-fleas are termed "water-shell-fleas"* (*Cypris*, &c.) and "sea-shell-fleas" (*Cythere*, *Cypridina*, &c.), having more distinct shells, with two valves, jointed on the back-line. These are abundant enough fossil in many strata of different ages.

The English names, however, are too general for special use in indicating the different kinds, the family and generic distinctions being very numerous and decided in this important sub-class of the great Crustacean group or class. Indeed it comprises these and other many-featured, minute, aquatic, articulate creatures, for which the term *Entomostraca* has been accepted as the systematic name.

Of these the really bivalved forms (namely, the *Ostracoda* and some of the *Phyllopoda*) have left their carapace-valves in the muds of lakes and seas of all ages as far as the history of the globe can be traced back geologically. The geologist recognizes three main stages of the world's progress, which he terms the Palæozoic (old life) or Primary, the Mesozoic (middle life) or Secondary, and the Cænozoic (new life) or Tertiary and Post-tertiary. These periods had their own characters of life, as shown by the fossils preserved in the respective groups of successive strata; and as regards the Bivalved Entomostraca there are many noticeable facts as to the per-

* 'Catalogue of Crustacea,' in the British Museum, 12mo, 1850.

sistency of certain generic types (judging from the remaining carapace-valves), and as to the coexistence in the early forms of several

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EXPLANATION OF PLATE LXI.

[The small horizontal lines indicate the length of the valves when magnified two diameters, for comparison with Figs. 17 *a*—*c*.]

FIG. 1. *Bairdia curta*, M'Coy. Mountain-limestone; Carboniferous. From the original specimen, free of the matrix. 1 *a*. Left valve seen. 1 *b*. Ventral edge of the same valve. Magnified 15 diam.

„ 2. *Thlipsura corpulenta*, Jones and Holl. Woolhope beds; Upper Silurian. 2 *a*. Right valve seen. 2 *b*. Ventral aspect. 2 *c*. Posterior extremity. Magnified 15 diam.

„ 3. *Cythere Jukesiana*, Jones and Holl. Kildare limestone; Lower Silurian. 3 *a*. Right valve seen. 3 *b*. Ventral view. Magnified 15 diam.

„ 4. *Cytherella brevis*, nov. Carboniferous beds, six feet above the Eurypterus-limestone; Carluke, Scotland. Magnified 15 diam.

„ 5. *Cytherellina siliqua*, Jones. Upper Silurian. 5 *a*. Internal cast, with part of the shell, showing level outside and undulated interior. 5 *b*. Right valve. 5 *c*. Edge view. 5 *d*. End view. Magnified 15 diam.

„ 6. *Æchmina cuspidata*, Jones and Holl. Wenlock limestone; Upper Silurian. Right valve. Magnified 15 diam.

„ 7. *Carbonia Agnes*, Jones. Coal-measures; South Wales. 7 *a*. Left valve. 7 *b*. The internal cast of the muscle-spot and its neighbourhood, in the variety *rugulosa*. Magnified 70 diam. 7 *c*. The external aspect of the muscle-spot in *C. Agnes*. Magnified 70 diam.

„ 8. *Cypridina Phillipsiana*, nov. Mountain-limestone; Ireland. 8 *a*. Right valve. 8 *b*. Ventral edge. Magnified 3 diam.

„ 9. *Cypridella Koninckiana*, nov. Mountain-limestone; Ireland. 9 *a*. Right valve. 9 *b*. Ventral edge. Magnified 3 diam.

„ 10. *Cyprella subannulata*, nov. Mountain-limestone; Ireland. 10 *a*. Right valve. 10 *b*. Ventral edge. Magnified 3 diam.

„ 11. *Entomoconchus Scouleri*, M'Coy. Mountain-limestone; Yorkshire. Right valve in view. Magnified 3 diam.

„ 12. *Entomis divisa*, Jones. Upper Silurian; from near Builth, South Wales. The two valves exposed. Magnified 3 diam.

„ 13. *Entomis biconcentrica*, nov. Mountain-limestone; Ireland. 13 *a*. Left valve. 13 *b*. Ventral edge. Magnified 3 diam.

„ 14. *Primitia renulina*, Jones and Holl. Wenlock limestone. 14 *a*. Right valve seen. 14 *b*. Dorsal view. Magnified 15 diam.

„ 15. *Kirkbya Urei*, Jones. Carboniferous; Scotland. 15 *a*. Side view. 15 *b*. Ventral view. Magnified 5 diam.

„ 16. *Moorea silurica*, Jones and Holl. Upper Ludlow rocks; Malvern. 16 *a*. Left valve. 16 *b*. Ventral edge. Magnified 15 diam.

„ 17. *Leperditia Balthica*, Hisinger, sp. Upper Silurian; Gothland. 17 *a*. Perfect carapace, left valve seen. 17 *b*. Dorsal view. 17 *c*. Front end. Magnified 2 diam.

„ 18. *Isochilina gracilis*, Jones. Lower Silurian of Isle Jesus, Canada. 18 *a*. Right valve. 18 *b*. Ventral edge. 18 *c*. End view. Magnified 5 diam.

„ 19. *Beyrichia Wilckenziana*, Jones. Upper Silurian of Gothland. Right valve. Magnified 5 diam.

„ 20. *Beyrichia Kloedeni*, M'Coy. Upper Silurian; Shropshire. Right valve. Magnified 5 diam.

„ 21. *Beyrichia complicata*, Salter. Caradoc and Bala works; Wales. Right valve. Magnified 5 diam.

„ 22. *Leaia Leidyi*, Lea, sp. Coal-measures; South Wales. Right valve. Magnified 5 diam.

„ 23. *Estheria membranacea*, Pacht, sp. Devonian; Livonia. Left valve. Magnified 5 diam.

„ 24. *Estheria tenella*, Jordan, sp. Carboniferous; Lanarkshire. Right valve. Magnified 5 diam.

characters not present together in later forms, but to be found in separate families and genera.

The Palæozoic strata (grouped as Laurentian, Cambrian, Silurian, Devonian, Carboniferous, and Permian) have yielded many genera of Bivalved Entomostraca, most of which have been described and figured; but some remain unpublished. Among these old bivalved carapaces and separate valves there are many that present such shapes and features as now belong to *Cythere*, *Cypris*, *Cypridina*, and their congeners; and it is very difficult, if not impossible, to form definite conclusions on imperfect evidence as to the exact grouping that should be adopted, in referring the fossil valves either to recent or to extinct genera; for, as has often been noticed, the general aspect of such carapaces is not only an ambiguous, but even a false guide; the minute distinctions of hingement, overlap, muscle-spot, and ornament being obscure, and the important characters of limbs and internal organs wholly wanting.

The following Table gives an outline of the classification of the Entomostraca: *—

CLASS CRUSTACEA.

[NOTE.—Those marked with an *asterisk* are known only in the fossil state.]

SUB-CLASS 1. DECAPODA (Cancer, &c.).
2. TETRADECAPODA (Oniscus, &c., Trilobita?*).
3. ENTOMOSTRACA.

ORDER I. Gnathostomata.

SUB-ORDER 1. Lophyropoda.

TRIBE I. Cyclopoidea (Copepoda).
Families. Cyclopidæ, &c.

TRIBE II. Daphnoidea (Cladocera).
Families. Penilidæ, Daphniadæ, Bosminidæ, Lynceidæ, &c.

TRIBE III. Cyproidea (Ostracoda). [*Bivalved.*]

Family 1. Cypridæ.

Genus. Cypris.	*Genus.* Notodromas.
Chlamydotheca.	Candona.
Cypridopsis.	Pontocypris.
Potamocypris.	Argilloecia.
Paracypris.	Bairdia.
Aglaia.	Macrocypris.

* The recent works of Mr. G. S. Brady should be consulted for the newest information on the living Ostracoda.

Family 2. Cytheridæ.

Genus. Cythere.	*Genus.* Bythocythere.
Limnicythere.	Pseudocythere.
Cytheridea.	Cytherideis.
Eucythere.	Sclerochilus.
Ilyobates.	Xiphichilus.
Loxoconcha.	Paradoxostoma.
Xestoleberis.	
Cytherura.	Thlipsura.*
Cytheropteron.	Carbonia.*

Family 3. Cypridinadæ.

Genus. Cypridina.	*Genus.* Eurypylus.
Asterope.	
Philomedes.	Cypridella.*
Cylindroleberis.	Cyprella.*
Bradycinetus.	Entomis.*

Family 4. Entomoconchidæ.

Genus. Entomoconchus.*	*Genus.* Heterodesmus.

Family 5. Conchœciadæ.

Genus. Conchœcia.	*Genus.* Halocypris.

Family 6. Polycopidæ.

Genus. Polycope.

Family 7. Cytherellidæ.

Genus. Cytherella.	*Genus.* Æchmina.*
Cytherellina.*	

Family 8. Leperditiadæ.*

Genus. Leperditia.*	*Genus.* Beyrichia.*
Isochilina.*	Kirkbya.*
Primitia.*	Moorea.*

Sub-Order 2. Phyllopoda.

Tribe I. Artemoidea.

Family 1. Artemiadæ.

Genera. Artemia, Chirocephalus, &c.

Family 2. Nebaliadæ.

Genus. Nebalia.	*Genus.* Peltocaris.*
Hymenocaris.*	Myocaris.*
Ceratiocaris.*	Ribeiria (?).*
Dictyocaris.*	

Tribe II. Apodoidea.

Family. Apodidæ.

Genus. Apus.	*Genus.* Dithyrocaris.*

TRIBE III. Limnadoidea [*Bivalved*].

Family. Limnadiadæ.

Genus. Limnadia.	*Genus*. Limnetis.
Estheria.	Leaia.*

ORDER II. Cormostomata.

SUB-ORDER 1. Pœcilopoda (Caligus, &c.).
2. Pycnogonoidea (Cyamus, &c.).

ORDER III. Merostomata.

SUB-ORDER 1. Eurypteridæ * (Pterygotus,* Eurypterus,* &c.).
2. Xiphosura (Belinurus,* Prestwichia,* Limulus, Neolimulus,* &c.).

SUB-CLASS 4. CIRRIPEDIA.
5. ROTATORIA.

THE LEPERDITIADÆ.

1. *Leperditia* (Fig. 17) is one of the largest and most common forms of the Palæozoic Bivalved Entomostraca. Indeed it is found only in the Lower and Upper Silurian, Devonian, and Carboniferous strata of different parts of the world. The earliest species seem to be *L. Canadensis* and *L. Anna*, small and variable, of early Silurian age. In the Upper Silurian strata much larger forms occur, especially *L. Balthica*, often almost an inch long, and sometimes larger. About twenty-four described species are recorded from the Silurian rocks. Others are mentioned, but the determinations appear uncertain. Three or four species are known from the Devonian rocks, chiefly of North America. In the Carboniferous period *L. Okeni* played an important part, both with a goodly growth in the open seas, and as dwarfed forms, with one variety or more for every locality, in the salt-marshes and lagoons among the coal-jungles, where layer after layer of the old mud-beds abound with the carapaces of these little water-scavengers of the period. Three or four other species of *Leperditia* are known in the coal-shales of Scotland. This genus was instituted in 1851 by M. Rouault. Its bivalved carapace is smooth, convex, horny in appearance, often brownish, sub-oblong or somewhat semi-ovate in outline, reminding one of some beans and tamarind-seeds; longer than broad (or high), inequilateral; posterior half broadest; dorsal border straight; ventral border nearly semicircular; anterior and posterior borders oblique above, rounded below, the valve-margin passing from each end of the hinge-line in an oblique direction downwards and outwards to about half the breadth of the valve, where it meets the curved ends of the ventral border, and so forms the more or less angular extremities of the valve, the former of which is narrower and sharper than the latter. The valves are united along their upper (dorsal)

borders by a simple linear hinge; the two extremities of the hinge-border form angles with anterior and posterior borders in each valve. The right valve is larger than the left, being broader, and overlapping completely the ventral border of the opposite valve, and to some extent its anterior and posterior borders. The overlapping ventral border of the right valve forms a thick blunt keel to the closed carapace. Each valve is somewhat depressed towards the dorsal border; this border in the left valve is thicker than that of the right, and sometimes slightly overrides it. In one species at least* the dorsal edge of the left valve swells out considerably in the middle. The ventral margin of the left valve is turned suddenly inwards, forming a thin plate projecting into the cavity of the carapace. The line of junction of the inverted border, or ventral plate, and the outer surface of the valve is angular, and bears a slight sulcus and moulding, against which the overlapping edge of the right valve abuts. The dorsal or the ventral profile of the closed valves is usually elongate acute oval; the end view of the closed valves is more or less ovate.

Rather above and in front of the centre of each valve, and on its most convex portion, nearly all the species of the genus present a slightly raised, circular or suboval swelling, having a diameter of from $\frac{1}{5}$th to $\frac{1}{4}$th of the breadth of the valve's surface. This swelling is distinguishable by a local change of colour or of surface-condition, and marks the place of a corresponding rounded pit, excavated on the interior surface of the valve so deeply as to render the tissue of the valve at the swelling somewhat diaphanous. The cast of this pit on an inner cast of the valve is strongly marked, having a greater relative height than the external swelling. This has usually a reticulated appearance, resulting from vascular impressions on the test; and from a slight sulcus at the margin of the pit a set of delicate canals, tortuous and inosculating, excavated on the interior of the valve, radiate forwards, downwards, and backwards, gradually becoming fainter towards the edges of the valves. This central tubercle, "lucid-spot," or "muscle-spot," marks the spot to which the great internal, closing or adductor muscle was attached. Anterior to it and nearer to the dorsal margin, a smaller but prominent tubercle is often visible on each valve, with a corresponding internal pit. This little anterior tubercle, or "eye-spot," corresponded to the place of the eye in the animal, and is usually seated on or at the edge of a slightly raised area, irregular in outline; and behind it a short shallow vertical sulcus, commencing at the dorsal margin, is usually apparent.

In the 'Annals Nat. Hist.,' 1856, ser. 2, vol. xvii., pages 96, &c., when first treating of *Leperditia*, I offered some observations on

* *L. gibbera*, Jones, 'Ann. Nat. Hist.,' ser. 2, vol. xvii., p. 90, pl. 7, figs. 8–10; and ibid., ser. 3, vol. i., p. 250, pl. 10, figs. 7, 10, and 11.

the probable family alliances of this palæozoic genus, remarking that we find some of the characters of its *carapace* among the existing Bivalved Entomostraca, both of the Phyllopod and Lophyropod groups, whilst others of its peculiarities have not been traced. It is not well represented by any known recent form, but partakes of the characters of several. Without again treating in detail of the shape, muscle-spot, and eye-spot, I will here indicate by such a table as I used before, but somewhat improved, the points of resemblance between the carapace of members of the recent families of Entomostraca and that of *Leperditia* (+ meaning strong resemblance or identity of structure; – showing a want of resemblance; and * a trace of likeness) :—

	Ostracoda.						Phyllopoda.
	Cypridæ.	Cytheridæ.	Cypridinadæ.	Entomoconchidæ.	Conchœciadæ.	Cytherellidæ.	Limnadiadæ.
Resemblance in outline ..	*	*	*	*	*	*	*
Muscle-scar: lucid spots	+	+	+	+	+	+	+
——— vascular sinuses ..	–	–	–	–	–	–	+
——— radiating marks ..	–	–	–	–	–	–	+
Situation of the eyes ..	+	+	+	* ?	–	* ?	–
Eye-tubercles	*	–	–	–	–	–	–

On a review of the facts of the case, it seems to me that the relationship of *Leperditia* is stronger with the Ostracods than with the Phyllopods, judging from the carapace alone; and I take this opportunity of stating my views. None of the recent Bivalved Entomostraca resemble it closely as to the carapace; and though some young Limnadians present such an *outline* in their valves, we must not count much on that circumstance, for the other features of the valves are different, and the mode of growth of the Limnadian carapace is very different. Among the Entomostraca all the bivalves necessarily have the sub-central transverse muscle, with its scar on either valve. This is indicated by some kind or other of "lucid spots" in all; and certainly the associated vascular sinuses and radii (branchial canals) seen in *Leperditia* have their strongest analogues in *Limnetis* among the Limnadians. To whatever extent this may be an essential character of the test, it cannot be of so much systematic importance as the relative situation of the eye of the animal, which in *Leperditia* the anterior tubercle indicates to have been upwards and forwards, as in the Ostracodans generally, whether they have in the valve an external indication of that organ (as in some *Cypridæ*) or not.

Thus, in this ancient animal, as in others, we find characters associated together which in modern forms are to be looked for in separate genera and families. And if it be so with regard to the

carapace, it was probably the case also with the internal organs; and after all, whatever classification we may adopt for such extinct types, it can only be provisional.

2. *Isochilina* (Fig. 18).—Some Lower Silurian Entomostraca from Canada, less than half the size of *Leperditia Balthica*, and differing from *Leperditia* chiefly in being equivalved, the margins of the valves meeting uniformly and not overlapping, were separated by me in 1858 as belonging probably to a sub-genus.* They are possibly members of a distinct genus. There were two species noticed, *Isochilina Ottawa* and *I. gracilis* (see pl. 10, figs. 1 and 2, *loc. cit.*).

3. *Primitia Solvensis* (Jones) and *P.* (?) *punctatissima* (Salter, MS.) are the oldest Bivalved Entomostraca as yet recorded. They are from the Lingula-flags, or lowest division of the great Silurian system of strata. Both of these little fossils were at first regarded as minute *Leperditiæ*, on account of a resemblance in shape; and several of their allies, from other old Silurian rocks of Wales and Canada, were in 1855 grouped as representing the simplest type of *Beyrichia*, the characteristic furrowing being absent except the apparently essential nuchal sulcus.† In 1865, however, these simple or unisulcate forms (*Beyrichiæ simplices* of 1855) were separated generically, because among the increased number of specimens we found a persistent occurrence of the chief features, with a passage towards *Leperditia*, by the loss of the furrow, rather than towards the bisulcate or real *Beyrichiæ*.‡ The *Primitiæ* have a minute bivalved carapace, equivalved or nearly so, convex, more or less oblong, often approaching *Leperditia* in shape by the sloping of the dorsal angles; hinge-line straight, sometimes nearly as long as the valve. The surface of each valve is usually impressed on the dorsal region, either medially or towards the anterior extremity, with a vertical sulcus, variable in size, sometimes barely visible, sometimes passing into, or even merely represented by, a navel-like pit; and sometimes the sides of the sulcus are swollen, and even raised up into tubercles. The ventral edges of the valves in some old individuals bear well-defined and even double marginal keels. A list of all the known *Primitiæ* is given in 'Ann. Nat. Hist.,' ser. 4, vol. xvi., p. 222, &c. There have been thirty-nine species as yet recognized; thirty-three of these were met with in the British Isles. The genus appears to be limited to the Silurian period (see Fig. 14).

4. The *Beyrichiæ* (Figs. 19–21) are very abundant throughout the Palæozoic series, from the lowest Silurian to the Carboniferous strata. They have small oblong or semi-ovate valves, strongly lobed

* 'Ann. Nat. Hist.,' ser. 3, vol. i., p. 248.
† Ibid., ser. 2, vol. xvi., pp. 85, 90, 171, &c.
‡ 'Ann. Nat. Hist.,' ser. 3, vol. xvi., p. 144.

and tubercled by deep transverse furrows. The amount of sulcation varies very much. Some unisulcate allies, formerly termed *Beyrichiæ simplices* (before alluded to), of simple outline, and passing into small nonsulcate Leperditioid forms, have been separated as *Primitiæ*. The *Beyrichiæ* proper (*B. jugosæ*) have the surface of the valve impressed with two or three strong vertical furrows, extending from the back to the ventral portion of the valve, and dividing the surface into three or more unsymmetrical lobes, transverse ridges, or bosses, which vary considerably in size, mode of subdivision, and relative position in different species, and to some extent in different stages of growth of individuals. The anterior, inferior, and posterior margins of each valve are turned sharply inwards, the angle so made being marked externally by a prominent ledge, either slightly rounded or trenchant (and sometimes spiny), forming a narrow compressed border along these three edges of the external surface of the valve. These edges of the valves close together by the marginal flange of the one valve being received within that of the other. The valves are almost equal in size, the ventral edge of the left valve very slightly overlapping that of the right. The surface is frequently marked with pittings, pimples, or other ornament. In some species (*B. Kloedeni*, *B. tuberculata*, *B. Buchiana*, and *B. impendens*) the antero-ventral lobe has been found to be enormously distended in old specimens.*

One group of the *Beyrichiæ*, typified by *B. Wilckensiana* (Fig. 19) and its variety *plicata*, are recognized as the *Corrugatæ*. They have the surface of the valve convex, impressed with two vertical furrows, not reaching across the valve, but marking out three unequal gibbous lobes. There is also a third, smaller furrow, defining a narrow semilune at the anterior end of the valve; and indeed there are two such small parallel vertical furrows in the variety (or species ?) *plicata*.

There are thirty-nine recorded species of *Beyrichia* from the Silurian, only two from the Devonian, and seven from the Carboniferous strata.

5. In *Kirkbya* (Fig. 15) the carapace-valves are flattish, thick, oblong, impressed with a subcentral pit, and raised into ridges, some concentric with the margin, associated sometimes with longitudinal riblets or wrinkles, and often accompanied by a reticulate ornament. In shape the valves are suboblong, usually higher behind than before; the extremities are more or less rounded, but one often much more obliquely than the other; the dorsal border is straight, and its ends are subacute; the ventral border is nearly straight in its middle third, and boldly curved at the ends; the hinge is simple. The ventral edge of the dextral valve overlaps slightly that of the other. The subcentral pit or sulcus is sometimes above and some-

* Jones, "Pal. Biv. Entom.," Geol. Assoc. Proc., 1869.

times below the median line of the valve, and varies greatly in its relative size. It appears to have reference to the nuchal furrow of *Beyrichia*, *Primitia*, and *Leperditia*. *Kirkbya* having relationships with *Beyrichia*, *Primitia*, *Moorea*, and *Leperditia*, is one of the *Leperditiadæ*.* To *Leperditia* it is related through *Beyrichia* and *Primitia*. In general form, hingement, ventral overlap, and even sometimes in a faint bilobation of the surface, the valves of *Kirkbya* resemble those of *Beyrichia*; but the double and sometimes threefold ventral rims, and especially the subcentral pit and the longitudinal riblets, distinguish them. The ventral ridges, to some extent, and the pit have their analogues in *Primitia*; but this genus generally presents convex forms; and when flattish its valves, though sunken (as in *P. excavata*), have no costation. *Moorea* presents flattish valves, marginally ridged, but without any subcentral pit or dorsal furrow.

The oldest known *Kirkbya* is *K. fibula* of the Ludlow division of the Silurian rocks.† No other has yet been found in this geological system, nor any in the Devonian rocks; but some Carboniferous strata at home and abroad abound with individuals belonging to one or other of ten species; and at least one form, with six varieties or very closely allied species, is plentiful in the Permian limestones of Europe and Britain.

6. *Moorea* is a Palæozoic Bivalved Entomostracan (previously referred to) with compressed valves, smooth or punctate, bounded more or less completely by a raised marginal ridge as in some *Kirkbyæ*, but not marked by any trace of a dorsal sulcus or subcentral pit. The Upper Silurian strata have yielded one (Fig. 16), and the Carboniferous Limestone two species of this genus.

III.—*On the Real Nature of Disease Germs.* By Lionel S. Beale, F.R.S., Fellow of the Royal College of Physicians, and Physician to King's College Hospital.

Before I proceed to describe the characters of the particles suspended in animal fluids, having virulent contagious properties, it is very desirable to draw attention to the minute particles of bioplasm, which may be demonstrated in many specimens of simple exudation. From this subject we shall pass on to the consideration of other forms of "exudation" which possess specific disease-producing properties. We shall find that by a careful microscopical examination of fluids which experience has proved to us have contagious properties, facts of great interest are disclosed which have an

* See 'Ann. Nat. Hist.,' ser. 2, vol. xvii., p. 99.
† Ibid., ser. 4, vol. iii., p. 224, pl. 15, fig. 9.

important bearing on the question of the nature of the poison of contagious diseases. Many such fluids are clear like water, and quite as transparent when examined by the unaided eye only. When we come to subject them to examination with the aid even of the highest powers yet made, although solid particles are detected, and sometimes in great number, we observe nothing peculiar to these fluids alone—nothing which would enable us to form any conception of the wonderful properties they possess—nothing that would attract the attention of the chance observer, or excite the interest of anyone who had not long and carefully studied the matter. Nevertheless, what we are able to demonstrate is of vast importance, and with the aid of other observations and experiments, we may form, I think, clear notions of the nature and origin of these morbid poisons, and of the manner in which they produce their marvellous and oftentimes disastrous effects. Much yet remains to be disclosed, but we shall soon learn more if we will but work and think independently, and accept the teaching of facts of observation and experiment, while careful to avoid being misled by the dogmatism of those who obstinately persist in asserting that *all vital phenomena* are to be explained by physics and chemistry, and try to make people believe that living organisms are mere machines constructed by force. All truly *vital* phenomena must necessarily be altogether out of the range of mere physical investigation. Nevertheless, to such extravagant lengths has the opposite view been carried of late, that it has even been seriously stated that he who refuses to look upon life as mere inorganic force, opposes investigation, and looks upon the structure of man's organism as a subject unsuitable for scientific exploration. It would be as reasonable to assert that a man who is to be a scientific investigator must commence by confessing his belief in the truth of a conclusion which has long been proved to be false by reason and observation.*

In spite of all the evidence adduced to the contrary, and notwithstanding the failure of all attempts to prove the vegetable nature of disease germs, Mr. Simon still adheres to this doctrine. He seems to place great reliance upon the extraordinary statements of Hallier, which have not been confirmed, and ought to be received with the greatest caution. In his Report for 1869, Mr. Simon makes the following remarks upon this subject:—"Knowing that all contagia (as such) are distinct one from the other, and believing that each of them has its essence in the so-called microzymes

* The physical theory of life is, however, less popular now than it was a few years ago. Quite recently its strongest advocates have expressed themselves more cautiously than has been their custom hitherto. They have discovered that the "tendency of thought" is fast setting in another direction. They have, therefore, modified their statements without withdrawing them, and have made preparations for abandoning their position without acknowledging defeat.

which it contains, we by implication impute to the microzymes that in different diseases they are not identical; and as we affirm them to be dynamically different, so also we assume that, under well-devised differential experiments, other signs of their specificity may be brought to light, and for each sort of them a definite genesiology be written." The learned author of this sentence appears to have accepted the doctrine that each contagious disease is produced by a specific vegetable organism. Now it appears to me that the arguments upon which this view is based break down as soon as they are analyzed and the facts advanced in their favour carefully investigated. This part of the subject has been already considered in my work 'On Disease Germs: their Supposed Nature.' It was also discussed in my Report on the Cattle Plague, printed in 1866.

I will now pass on to consider the actual nature of the living germs in various "exudations," some specimens of which were described and figured by me as long ago as 1863.

Simple Exudation.—The transparent colourless fluid which moistens the surface of a superficial wound after it has ceased to bleed, is poured out from the capillaries, or from the lymphatic vessels, or from both sets of vessels. This fluid, besides containing albumen in solution, contains multitudes of minute particles of bioplasm, which grow and multiply outside as well as within the vessels. These form fibrin and matters more or less allied to it, and perform an essential part in the healing process, or in the formation of pus, as the case may be. These minute particles of living matter are present in the blood and lymph in countless numbers. They are concerned in the production of fibrous tissue outside the capillaries, a change which occurs in many pathological processes, and also in the production of pus-corpuscles, and other "corpuscles" in the same situation, in disease. All "exudations" contain these particles of living matter. The following paragraphs are taken from a paper written by me in 1863:*—

"When the capillary vessels are distended, as in that extreme congestion which soon passes into inflammation, a fluid which possesses coagulable properties transudes through the stretched capillary walls. It is probable that in such cases minute and narrow fissures result, which, however, are too narrow to allow an ordinary white or red blood-corpuscle to escape, but, nevertheless, wide enough to permit many of the minute particles of the living or germinal matter (the existence of which in the blood has been already referred to) to pass through. The small protrusions upon the surface of the white blood-corpuscle might grow through the capillary walls, become detached, and pass into the tissue external

* "On the Germinal Matter of the Blood, with remarks upon the Formation of Fibrin." Microscopical Society, December 9th, 1863. See 'Trans. Mic. Soc.,' April, 1864.

to the vessels. Such minute particles of living matter external to the vessels being surrounded with nutrient pabulum, and stationary, would grow and multiply rapidly, while a similar change would of course go on in the now stagnant fluid in the interior of the capillary. The result would be exactly that which is observed, *viz.* the presence of a vast number of cells like white blood-corpuscles in the *interior of the capillary vessel, and immediately around its external surface*, and sometimes these extend for some distance in the substance of the surrounding tissue, and they increase in number.

"I venture, then, to conclude that many of the clear fluids which have been considered as '*exudations*' from the blood, really contain a multitude of extremely minute particles of living matter, which are intimately related to the white blood-corpuscles, and that these grow and become one source of the small granular cells or corpuscles which are so familiar to all who have studied morbid changes in the tissues as they occur in man and the higher animals.

"Some of these active living particles may be so small as to be invisible by a power magnifying 5000 diameters. I have seen such particles, less than the 50,000th of an inch in diameter, and have no reason whatever for assuming that these are really the smallest that exist."

These minute particles of bioplasm multiply freely, but they may also be derived from the white blood-corpuscles, and from other forms of bioplasm. As the blood coagulates they undergo change, die, and help to form the non-living fibrin. In every clot numerous white blood-corpuscles, also composed of living matter, can be detected. In coagulation it is probable that the most minute particles of bioplasm change first, and become fibrin. After a time the white blood-corpuscles also die, and thus the coagulum of fibrin continues to increase for a short time after coagulation has commenced. The lines round the red blood-corpuscles seen stretching from one to the other in a drop of coagulating blood indicate the earliest stage in the formation of fibrin. The minute particles of bioplasm may be seen actually undergoing change. The bioplasm of the blood is derived from the bioplasm originally found in the vessels of the germinal area at a very early period of development, from the bioplasm of the capillary walls, which is very abundant in some capillaries, and projects into the cavity, and from the lymph and chyle bioplasm which is being continually poured into the vascular system and mixed with the blood.

If the clear transparent material which moves round the cells of *Vallisneria* and other plants be carefully examined under very high powers magnifying upwards of 2000 diameters, it will be discovered that this is not a simple fluid like water containing the nucleus and chlorophyl. But the apparent fluid has suspended in it an infinite number of particles of living matter, like those of

which the amœba, white blood-corpuscle, and other forms of living matter consist. With high powers the slightly opalescent appearance may be detected, and by careful focussing, the minute particles of living matter will be brought into view. The movements of the fluid may therefore be compared with the movements of the living bioplasm of an amœba. In the circulating juice of many plants similar appearances may be observed, and in the blood and circulating fluid of all animals, and in man himself, minute particles of living matter are to be demonstrated in immense multitudes. These are diffused through the fluid, and to them is probably due the movement of the contents of the finer vessels and spaces. This constituent of the blood, seen with such difficulty that its presence is not yet admitted by observers, is probably the most important, for its increase or diminution may occasion serious disease or death. This almost impalpable living moving matter is the seat of many very important changes, and is perhaps influenced before any other constituents of the body when certain poisons and disease germs find their way into the blood. "Protection," after successful vaccination, and the escape from a second attack, which is the rule in the case of many contagious fevers, is most likely brought about by changes induced in the living matter under consideration.

In health it is upon this material that the coagulable property of the blood is mainly dependent, and it is this which in great part undergoes conversion into what we call fibrin, when the blood is removed from the living vessels or "dies." If destroyed it may, under favourable circumstances, be renewed by the appropriation of nutrient matter. The white blood-corpuscles are intimately related to this living bioplasm, and take part in its formation. I believe they bear to it the same relation as the "nucleus" in the cell of Vallisneria bears to the living particles suspended in the fluid, while the red blood-corpuscles of the blood correspond to the chlorophyl particles in the rotating fluid contents of the vegetable cell. Attention will presently be drawn to the vast importance of this living fibrin-forming matter in various exudations, and it will be found that a simple explanation of many most important morbid phenomena may be given. Now in the fluid exudation or virus which produces a "poisoned wound" when inoculated we also find minute particles of living bioplasm.

Many arguments will be advanced herein in favour of the view that the virulence of the poison is due entirely to the living particles, and not to the fluid in which these are suspended. In the case of some of these poisonous fluids we are able to study the production of the contagious virus, and we may even in some cases succeed in tracing out the manner in which the material with the wonderful poisonous property originates.

In some forms of inflammation of serous membranes the process

may be made out, and a conception formed of the several changes which occur, and at last end in the development of the poison. The morbid change is sometimes limited to the effusion of serum and the production of "inflammatory lymph," but in other instances the inflammation proceeds to a further stage, and actual pus is generated. Peritonitis is an example of an inflammation which much more frequently proceeds to the formation of pus than inflammation of other serous membranes. The greater vascularity of the peritoneum as compared with allied textures may perhaps account for this fact. It is interesting to discuss briefly the characters of the different "inflammatory products," as they are called, resulting from peritoneal inflammation, varying in intensity.

In *slight inflammation* there is great vascular distension, accompanied as in other cases by the escape of exudation in which are suspended particles of bioplasm. The exudation coagulates upon the surfaces of the serous membrane, perhaps glueing them together. The fluid portion is gradually absorbed, and if the case progresses to recovery, much of the coagulated matter is also taken up, a little being transformed into fibrous tissue, resulting in a few "adhesions," or mere thickening of the serous membrane, as the case may be.

When, however, the intensity of the inflammation is more marked, the little particles of bioplasm originally derived from the white blood-corpuscles, grow and multiply, and with the fibrinous matter in which they are entangled form transparent flocculi, which are suspended in the serous part of the exudation, or adhere here and there loosely to the peritoneal surface. Many of these flocculi are found to contain multitudes of bioplasm particles, and oftentimes a vast number of these are suspended in the fluid, and congregated here and there, form little collections upon the surface of the delicate serous membrane, to which they adhere and where they grow.

If the inflammatory process still continues, and increases in severity, the vascular congestion becomes more marked, and the exudation is poured out from the blood more abundantly; the masses of bioplasm increase in number yet faster, and the exudation in consequence appears nearly opaque. The flocculi are of a yellowish colour, and look very like pieces of clotted cream which stick here and there to the peritoneum, covering the intestines and the inner surface of the abdominal parietes. Not unfrequently the surface is smeared over in places with whitish pasty masses of soft cream-like matter, in the intervals between which the highly-injected vessels stand out with great distinctness. The masses of bioplasm would now be called *pus-corpuscles*. Here, then, is an interesting example of *the production of pus-corpuscles by the rapid growth and multiplication of particles of bioplasm which*

were once in the blood, and intimately related to the white blood-corpuscles.

But further: if, as is well known, a little of this material were to be introduced into the body, as may unfortunately happen from a dissection-wound in the course of making a post-mortem examination, terrible inflammation may be excited in the person inoculated. The most tiny morsel of this virulent, rapidly-multiplying morbid bioplasm, may give rise to a dreadful form of "blood-poisoning," which may end fatally and in a very short time.

In some cases similar poisonous particles which have been derived from a diseased organism are so very minute and light that they are supported by the air, and may find their way into the blood of a healthy (?) person through his respiratory organs, or may gain access to his circulating fluid by traversing the narrow chinks between the epithelial cells of the cuticle.

Nature of Virulent Disease Germs.—Now, what is the nature of the matter inoculated, which produces these dreadful results? The virulent poison which sometimes destroys life in cases of dissection-wounds cannot be attributed to the presence of vegetable germs, for the period of its most virulent activity is very soon after death, but before the occurrence of putrefaction, when the vegetable fungus germs multiply.* A punctured wound is not dangerous if putrefactive decomposition has taken place, because, although bacteria are developed in immense numbers, *the real contagious virus is dead.* The vegetable germs, in fact, grow and flourish upon the products resulting from the death of the dangerous animal living poison. In short, this material is living and very actively-growing germinal matter; living matter which retains its life after the death of the organism in which it was produced has occurred; living matter which *has descended directly from the living matter of health*, but which has acquired the property of retaining its life under new conditions; living matter destroyed with difficulty, and possessing such wonderful energy that it will grow and multiply when removed from the seat of its development and transferred to another situation, provided only it be supplied with suitable nutrient pabulum—and it is to be feared the ordinary nutrient fluids of a perfectly healthy organism are eminently adapted for the nutrition of this destructive virus.

The Pus of Purulent Ophthalmia—Gonorrhœal Pus.—Such is the vitality of these forms of bioplasm that they will grow and multiply upon certain mucous surfaces if placed there; not only so, but the living particles will retain their vitality for some time after their removal from the surface upon which they grew. They may even be transported long distances by the air, or they may

* See 'Disease Germs: their Supposed Nature.'

remain for some time in moist cloths without being destroyed. When once a room has been infected with such particles, some weeks may elapse before the death of all the specific disease-carrying germs has taken place.

The pus possessing specific contagious properties cannot be distinguished from ordinary pus. It differs indeed from this last, but not in appearance, chemical composition, or physical properties. It differs in *vital power*.

Vaccine Lymph.—Vaccine lymph which has been just removed from the growing vesicle will be found to contain a great number of extremely minute particles of bioplasm, which may be well seen under a power magnifying from 1000 to 2000 diameters. In 1863 I made a drawing of the appearances I observed in the bioplasts from a drop of perfectly fresh lymph which had been transferred to a warm glass slide, and carefully covered with very thin glass, under the $\frac{1}{25}$th object-glass, which magnifies about 1800 diameters. The results are represented in the plate accompanying a memoir which was published in the 'Quarterly Journal of Microscopical Science' for April, 1864.

In vaccine lymph which has been kept for some time in glass tubes multitudes of very minute particles are observed, and these exhibit the most active molecular movements. These particles have often been termed *débris*, and have been regarded as quite unimportant elements of the lymph. To them, however, the active properties of the lymph are entirely and solely due. And I should be no more inclined, in the absence of the most positive evidence to the contrary, to regard the fluid portion of the vaccine lymph as the active material, than I should be to assume that the fluid in which the spermatozoa were suspended was the fertilizing agent, and that the spermatozoa themselves were merely epithelial *débris*, and quite unimportant; or to infer that the fluid in which the yeast fungi or bacteria were growing was the active agent in exciting fermentation, while the actually growing, moving, and multiplying particles were perfectly passive. The germinal particles in all cases are, without doubt, the active agents, and it seems to me as much opposed to the facts of the case to maintain that the *materies morbi* of cattle plague and other contagious fevers is a material that can be dissolved in fluid, and precipitated and re-formed, or sublimed as a volatile substance, as it would be to look upon any living organism as the result of the concentration of an albuminous solution, and capable of resolution and precipitation.

The little particles I have represented in my drawings could not be distinguished from the minute particles of living pus, or other germs of living germinal matter, and I think they consist of a peculiar kind of living matter, the smallest particle of which, when supplied with its proper pabulum, will grow and multiply,

giving rise to millions of little particles like itself, each having similar properties and powers.

I consider it to be almost certain that the material of which these particles are composed has the power of forming matter like itself from pabulum around it, which differs from it in properties and composition. Such living germs may pass from the organism on which they grew to another, and will grow and multiply there if they meet with the proper pabulum. The only condition in which matter is known to exhibit these powers of self-multiplication is the living state.

M. Chauveau* described these same bodies in 1868. It is evident he had not seen my observations, published in the Cattle-Plague Report, or my previous researches published in the Microscopical Transactions for 1863.† Chauveau showed that the active particles subsided after forty-eight hours, and that no effects were produced by inoculating the albuminous supernatant fluid, while the full effects were produced by vaccinating with the deposit. As would be supposed from the excessive minuteness of these bodies, they are not to be separated by ordinary filtration, but if the fluid containing them also contains a trace of coagulable fibrin diffused through it, this by contraction after coagulation would filter off the little bioplasts, and leave a serum perfectly free. Dr. Farr calls the living particles *biads* (βια, force, βιος, life), and speaks of the vaccine particles as *vaccinads*.‡ These words are better than *microzyme*, adopted by Mr. Simon and others, because they involve no theory save that the particles are *living*, while the latter term affirms them to be of the nature of a ferment acting like yeast.

The circumstance that vaccine lymph retains its activity if kept in a tube for several weeks, seems conclusive as to the possibility of the particles retaining their vitality for a considerable time after they have been removed from the place where they grew; the arguments advanced, as proving that the active power resides in the particles and not in the fluid, being admitted. It is not more difficult to explain the fact that such living particles may be dried without losing their power, than that an amœba or rotifer should exhibit the same peculiarity. As this property is observed in connection with many of the lower forms of life, we might almost anticipate that the living matter from the highest organisms, if reduced to a degraded condition, would retain its vitality under circumstances which would cause its death in its normal condition. Yet it must not be supposed that these particles, any more than the "dried animalcules," are really dried. Some moisture is re-

* 'Comptes Rendus,' February, 1868.

† "Beale had, before Chauveau, declared that the 'active properties of vaccine lymph are entirely and solely due' to these corpuscles. He has figured them."—Dr. Farr, 'Report on the Cholera Epidemic of 1866,' p. lxviii.

‡ 'Report on the Cholera Epidemic of 1866,' p. lxx.

tained by the particles within the imperfectly-dried mass. Complete desiccation will destroy life in both cases. Since it has been shown that the active powers of vaccine lymph reside in the minute particles of living germinal matter, and it has been proved that these may be dried (imperfectly) without loss of power, it is surely not too much to conclude that the *materies morbi* of other and allied contagious diseases is probably composed of living particles, which have the same property of living for some time in a state of partial desiccation.

Living Germs of Variola.—I have examined the contents of the little vesicle which rises in small-pox at different stages of its development, and find, as in allied pathological changes, vast multitudes of minute particles of living matter or bioplasm, but, as will have been anticipated from what has been already said, these present nothing peculiar or characteristic, nothing that would enable us to say if we saw these particles under the microscope that they were obtained from a small-pox vesicle, and would certainly give rise to that disease. I have made a drawing of some of the varioloid bioplasts from a well-developed vesicle on the fifth day of the disease, and also from a vesicle which was just making its appearance.

Living Germs of Fever.—As was shown experimentally by Dr. Sanderson, a mere trace of blood serum was sufficient to propagate cattle plague. A very small portion of blood or of the tissues of an infected animal had the same effect. Nay, the contagion is so subtle that in this as well as in many other contagious diseases, the breath of the diseased organism contains numbers of the potent particles of poison, and in this manner the very air of a considerable space or even district may become infected.

In the blood in the smaller vessels, as well as in the mucous secretions of the mouth, intestinal canal, and in the milk of animals suffering from this disorder, I have found multitudes of minute particles of bioplasm, which, as long as they remain alive, are, without doubt, disease-carrying particles.

The disease germs of many contagious fevers will retain their vitality in water and other fluids for a length of time, and there is reason for concluding that some of these poisons not only grow and multiply in media different from any in the organism, but that in the course of such growth and multiplication they acquire still more virulent properties. Dr. C. Macnamara has discovered that cholera poison in water after exposure to the sun for a few hours becomes extremely virulent, and that this period corresponds with the development of multitudes of vibrios; but that after the lapse of a day or two, when the vibrios will have disappeared and given place to ciliated animalcules, the fluid may be taken with impunity.

Syphilitic Disease Germs.—This is another of those remarkably special living poisons which may be suspended in serum and other fluids, and retain its vitality for a length of time.

There is reason for thinking that a single epithelial cell may carry multitudes of active particles of syphilitic poison, one of which introduced into the blood or lymph of a healthy person would probably grow and multiply, and give rise to pathological changes characteristic of, and quite peculiar to this particular poison.

We know that the syphilitic poison may retain its specific characters in the organism for years, from time to time giving rise to local pathological phenomena, which are characteristic of this kind of morbid bioplasm. It is impossible from the facts of the case to arrive at any other conclusion than this: that a certain portion of the living matter remains in the organism, and that under certain favourable circumstances this grows and multiplies, producing disease. Particles of this virulent poison may be transferred from the infected organism to a healthy one, and contaminate it, even many years after its introduction into the first had taken place. Of syphilitic bioplasm there are different kinds, giving rise to different pathological affections belonging to the syphilitic class. Indeed, some facts render it probable that there are several different species or varieties of syphilitic poison, instead of only one or two.

One very remarkable property of the poison of syphilis is, that it may be re-inoculated into the same organism over and over again, until inoculation ceases to produce any specific effect. As soon as this is the case, it is said the organism is "protected." But such protection sometimes cannot be procured until successive inoculation has been practised during several months, and, as has been remarked, the remedy is in many respects worse than the acquired disease, besides being, and on many grounds, quite unjustifiable.

Living Disease Germs in Secretions.—The living germs of many fevers pass from the blood into the secretions. The urine, the secretions from the mucous membrane of the nose, mouth, stomach, and intestinal canal, contain them in large numbers. There is reason to think they may also escape in the secretion of the sweat and sebaceous glands. In the excrements there can be no doubt disease germs exist in vast numbers in typhoid fever, in cholera, and in some other diseases. Even in the milk, in the tears, in the saliva, they are present. Some of the living particles of contagious (?) bioplasm in the milk from a cow suffering from cattle plague are represented in my Report, as well as particles of bioplasm and fungi in vaginal mucus from another animal suffering from the same disease. The spherical form, sharp, well-defined outline, and the high refractive power of the envelope of the fungi, positively distinguish them from disease germs.

Living tubercle germs will not be considered as very closely related to the contagious particles which are the active agents in the propagation of contagious fevers. There is, however, reason to think that particles of living growing tubercle exist sufficiently minute to be supported by the atmosphere and carried long distances; while there are many facts which are considered by some sufficiently conclusive to justify the opinion that tubercular disease of the lungs is at least in some instances *contagious*. And it is certain that the most recent observations in connection with the subject of the nature and mode of propagation of tubercle, so far from militating against this view, tend rather to support it. That tubercle is not eminently contagious is certain, while the probabilities of minute particles of living growing tubercle escaping into the air while it remains in the air-cells of the lungs, or rising in a living state into the atmosphere from the sputum after its expectoration, are not great. At the same time neither circumstance can be regarded as impossible, neither view held to be untenable.

The manner in which the bioplasm of tubercle multiplies may be studied in tubercular inflammation of the membranes of the brain:—The living matter may be seen extending round a small artery in the areolar tissue of the external coat. The living particles obstructed in the vessel make their way through its lining membrane and between the fibres of the muscular coat, until they reach the areolar tissue outside, where they grow and multiply. By their accumulation, the pressure upon the vessel becomes greater, and at last its calibre will be completely obliterated. From such tubercle collections minute germs may be readily detached, and after having found their way into a pervious lymphatic vessel, or blood capillary, might be carried to distant parts and grow there. In this way tubercles are formed in many different parts of the body and in the substance of many different tissues and organs. If a particle of fluid, holding tubercle germs in suspension, were introduced by inoculation into a healthy organism, the disease might be produced.

Cancer Germs.—While it is almost certain cancer might be introduced by direct inoculation into the organism of a healthy person, many circumstances render it in the highest degree improbable that living germs detached from the growth could, under any circumstances, gain access to another organism through the air breathed, or in any other manner pass into the blood or tissues, as long as the surface remained uninjured. Cancer germs would probably live for some time in animal fluids out of the body, and it is by no means impossible that we may succeed in growing them in glass vessels away from their natural seat of growth, and watch the changes which occur under our microscopes; but it is exceedingly doubtful if these germs would long retain their vitality if removed from the fluid which nourished them.

Between the cancer germ, which cannot be conveyed by the air from the diseased organism to one not infected, and the germ of scarlatina, which will retain its vitality for weeks after it has escaped from the organism in which it was produced, and may readily gain access to healthy organisms in the air they breathe, we have examples of living disease germs manifesting powers of retaining their vitality when free in many different degrees. In other words, these poisons differ remarkably in the facility with which they are propagated, or spread from person to person. All exhibit the same appearances, though they differ remarkably in power. The capacity for resisting death, due to some inherent power and not to their chemical composition, varies much, some being capable of living for weeks or months away from the fluids of the body, while others die within a very short time after their removal from the seat of growth.

[In this paper a number of remarkable forms of contagious matter or virus have been referred to. These resemble one another in general appearance. Neither by its form, chemical composition, or other demonstrable properties, could the vaccine germ be distinguished from the small-pox germ, or the pus germ from either. All resemble the minute particles of bioplasm of the blood from which they have probably been derived, but from which they differ so remarkably in *power*. Concerning the conditions under which these germs are produced, and of the manner in which the rapidly-multiplying matter acquires its new and marvellous specific powers, we have much to learn, but with vegetable organisms the germs have nothing to do. They have originated in man's organism. Man himself has imposed the conditions favourable to their development. Man alone is responsible for their origin. Human intelligence, energy, and self-sacrifice may succeed in extirpating them, and may discover the means of preventing the origin of new forms not now in existence.

IV.—*The Histology of Minute Blood-vessels.*

By Brevet Lieut.-Col. Woodward, Assist.-Surgeon, U. S. Army.

Having recently been occupied in the critical examination of certain preparations, in the Microscopical Section of the Museum, illustrative of the minute anatomy of the blood-vessels, I have thought that some of them threw so much light on certain points involved in the recent discussions with regard to the doctrine of inflammation, that a short account of them would be of interest, and might perhaps do good service, in connection with the appreciation of the conflicting statements which have appeared in the medical journals

since the publication of the paper of Dr. J. Cohnheim,* on inflammation and suppuration.

Perhaps the observations of Cohnheim must fairly be regarded as elaborations of the previous experiments of Dr. Augustus Waller, but certainly they produced an impression upon the medical world far beyond that made by the papers in the 'Philosophical Magazine,'† and more or less complete accounts of the conclusions arrived at by the distinguished Berlin observer have continued to appear, from time to time, in both foreign and American medical journals, ever since the publication of his paper in 1867.

Recently protests against these conclusions have appeared in various quarters, among which particular reference may be made to the paper of Prof. Koloman Balogh, of Pesth, published in 1869,‡ and that of Dr. V. Feltz, of Strasbourg, in 1870.§ Both these authors have failed to see the white blood-corpuscles pass through the coats of the small vessels in the manner described by Cohnheim, and deny the existence of stomata, between the cells of the vascular epithelium, large enough to permit such a wandering to occur.

After I had perused Cohnheim's paper I procured a number of frogs, and having on hand a small quantity of Wourara, the gift of my friend Dr. S. Weir Mitchell, of Philadelphia, I carefully repeated many of the experiments described. I received the impression from what I saw that Cohnheim was a most conscientious observer, who had described as faithfully as possible the impressions made upon him. Certainly the results I obtained, by following his methods of producing inflammation in the cornea and mesentery of frogs, could be described in his very language without drawing upon the imagination. Nevertheless my other duties did not leave me sufficient time for an exhaustive research in this difficult domain, and it is far from my present purpose to enter into a critical discussion of the subject. It is simply my desire to offer a brief description, illustrated, of certain preparations in the Microscopical Section of the Museum, which bear upon some of the points involved, and thus to contribute what is in my power towards the important object of

* "Ueber Entzündung und Eiterung," Virchow's 'Archiv,' Bd. XL., S. 1.

† "Microscopical Examination of some of the principal Tissues of the Animal Frame as observed in the Tongue of the Living Frog, Toad, &c.," 'London, Edinburgh, and Dublin Philosophical Magazine,' vol. xxix., p. 271 (1846). "Microscopical Observations on the Perforation of the Capillaries by the Corpuscles of the Blood, and on the Origin of Mucous and Pus Globules," ibid., p. 397.

‡ "In welchem Verhältnisse steht das Heraustreten der farblosen Blutzellen durch die unversehrten Gefässwandungen zu der Entzündung und Eiterung?" Virchow's 'Archiv,' Bd. XLV., S. 19. Readers inclined to attach importance to this paper should read the caustic criticism of Dr. Alexis Schklarewski of Moscow, ibid., Bd. XLVI., S. 116.

§ "Recherches Expérimentales sur le passage des Leucocytes à travers les parois vasculaires," 'Journal de l'Anatomie et de la Physiologie,' Jan. and Feb. 1870, p. 33.

arriving at certainty with regard to the facts on which our future theories of inflammation are to rest.

Most of the preparations here referred to are examples of the results attainable by staining the tissues with a dilute solution of the nitrate of silver. This reagent has been employed for various histological purposes during the last ten years, and has attracted attention especially in connection with the cornea, the various forms of connective tissue, the ultimate branches of the lymphatics and the boundaries of the cells which constitute epithelial surfaces. General attention was first drawn to its use by Dr. F. von Recklinghausen, of Berlin, in 1860,* and further particulars were contributed during 1861, by Prof. His, of Basel,† who would appear to have already employed the reagent for several years. In 1862 Von Recklinghausen published his work on the lymphatics,‡ which contains a detailed account of many elaborate experiments with regard to the action of silver solutions on the tissues, and in 1863 Dr. Ernst Oedmanson, of Stockholm,§ gave a description of their behaviour when applied to epithelial surfaces, and described and figured the so-called stomata which play so important a part in the theory of Cohnheim. During 1865 and 1866 the epithelium of the capillary blood-vessels, as shown by silver, was described by several observers, among whom Dr. N. Chrzonszczewsky, of Charkow,‖ may be particularly mentioned.

The perusal of these papers led me to make a number of experiments myself, and to have others made by my assistants at the Museum, the results of which are now to be described.

If a dilute solution of nitrate of silver is brushed over a clean epithelial surface taken from a recently killed animal, and the tissue after washing with distilled water is exposed for a short time to the action of sunlight, it will be found on microscopical examination that a brownish black precipitate of silver has been produced at the boundaries of the epithelial cells, while the cells themselves are comparatively but little stained, or if the manipulation has been care-

* "Eine Methode, mikroskopische hohle and solide Gebilde von einander zu unterscheiden," Virchow's 'Archiv,' Bd. XIX., S. 451.

† "Ueber das Verhalten des Salpetersauren Silberoxyds zu thierischen Gewebsbestandtheilen," ibid., Bd. XX., S. 207.

‡ 'Die Lymphgefässe und ihre Beziehung zum Bindegewebe,' Berlin, 1862.

§ "Beitrag zur Lehre von dem Epithel," Virchow's 'Archiv,' Bd. XXVIII., S. 361.

‖ "Ueber die feinere Structur der Blutcapillaren," ibid., Bd. XXXV., S. 169. C. J. Eberth, in his article on the blood-vessels in Stricker's Handbook ('Handbuch der Lehre von den Geweben, &c.' Leipsic, 1869. II. Lief: S. 202) enumerates the following microscopists as having described the epithelium of the capillaries prior to Chrzonszczewsky:—Hoyer, 'Archiv für Anatomie,' Jan. 18, 1865. Auerbach, 'Breslauer Zeitung,' Feb. 17, 1865. Eberth, 'Sitzungsberichte der physikal. med. Gesellschaft zu Würzburg,' Feb. 18, 1865; 'Medicinisches Centralblatt,' No. 13, 1865; 'Würzburger Naturwissenschaftliche Zeitschrift,' Bd. VI., 1866. Aeby, 'Medicinisches Centralblatt,' No. 14, 1865.

fully conducted, are not stained at all. For this purpose I have most frequently employed, at the Museum, a solution made by dissolving one part of crystallized nitrate of silver in four hundred parts of distilled water, but considerable variation on either side of this strength does not much modify the result, provided the solution is well washed off before the tissue is exposed to the light.

If the same solution is injected into the blood-vessels, the lining epithelium is handsomely mapped out in all those membranous and superficial parts in which a ready exposure to the action of light is practicable, and although in the parenchymatous organs, such as the liver, the spleen, the kidneys, &c., the juices of the tissues are apt to interfere with the reaction, yet even here occasional success may be attained. In practice it is often found advantageous to combine the silver solution, intended for injection, with a certain amount of gelatine, by which the blood-vessels are kept handsomely distended, and the beauty of the preparation is much increased. This plan was proposed by Chrzonszczewsky in the paper already quoted. His formula, which I have found to work well, is as follows:—Half an ounce of fine gelatine is dissolved in four ounces of distilled water, and to this is added a solution of one scruple of nitrate of silver in two fluid drachms of distilled water. After injecting with this fluid, the tissue is exposed to the light precisely as after the use of the simple silver solution.

There are preserved in the Microscopical Section of the Museum a number of silver stainings in which the epithelium has been thus mapped out on the skin, the peritoneum, the lymphatic sacs of frogs, and the blood-vessels. These preparations, after the action of the silver, have been mounted in Canada balsam with or without the previous staining of the nuclei with carmine. The detailed steps of the process may prove useful to some readers. The silver staining having been successfully accomplished, the nuclei are tinted preferably by the solution of carmine in borax, described by Thiersch in his work on epithelial cancer.* It is prepared as follows:—Four parts of borax are dissolved in fifty-six parts of distilled water, and one part of carmine added to the solution; one volume of this fluid is mixed with two volumes of absolute alcohol, and after crystals have formed the mixture is filtered. The filtrate may be used for staining, but if the crystals of carmine and borax which remain on the filter are dissolved in a small quantity of distilled water, I find the solution thus obtained answers a still better purpose. The portion of tissue to be studied is soaked in this solution until coloured deep red. It is afterwards treated with a saturated solution of oxalic acid in alcohol, by which all colour is gradually removed except from the nuclei. So soon as this is accomplished

* 'Der Epithelialkrebs,' Leipsic, 1865, S. 92.

the piece is to be carefully washed in alcohol, then soaked in absolute alcohol, and finally mounted in a solution of dried Canada balsam in chloroform or benzole. The treatment by oxalic acid, subsequently to the action of the carmine-borax solution, has the additional effect of altering the purplish-red colour, derived from that fluid, to the brilliant hue obtained by the use of the ammoniacal solution of carmine ordinarily employed in histology. The latter has the disadvantage of being apt to dissolve out the previously produced silver staining, an annoyance completely avoided by the carmine-borax solution.

Preparations carefully made by the above process closely resemble the fresh tissues, as they appear after staining if immersed in glycerine or syrup: they are somewhat more transparent, but not inconveniently so, and possess the great advantage of keeping unchanged for an indefinite period of time.

After these preliminary remarks, I proceed at once to the description of the photographs.

[It will be observed that we have omitted a great number of photographs, as woodcuts of them have not been published.]

I. Sketch of a photograph representing several venous radicles

uniting to form a small vein, in the muscular coat of the urinary bladder of the frog. Negative No. 102, New Series. From preparation No. 3378, Microscopical Section. Magnified 400 diameters

by Wales's $\frac{1}{8}$th objective, illuminated by the magnesium lamp. The preparation was made by Dr. J. C. W. Kennon.

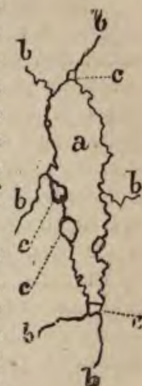
The principal venous trunk represented in this photograph is $\frac{1}{400}$th of an inch in diameter. It is formed by the union of three smaller radicles, of which that on the left hand is much out of focus. Another smaller radicle, also much out of focus, joins the trunk on the left, near the bottom of the picture. The walls of the venous trunk, and of those of its branches which are in focus, are plainly seen to be formed of somewhat irregular epithelial cells, which vary in shape and size, averaging $\frac{1}{500}$th of an inch in length and $\frac{1}{2200}$th in breadth. The boundary of each cell is indicated by a zigzag black line. In each of the cells which is accurately in focus, a smooth, oval nucleus, $\frac{1}{2800}$th of an inch in length is visible. In examining the original preparation, by changes in the fine adjustment of the microscope, similar nuclei can be seen in each of the epithelial cells. These nuclei, being brilliantly stained with carmine, contrast sharply with the black cell-boundaries resulting from the silver imbibition. By a still further alteration of the fine adjustment, the cells and nuclei of the opposite side of the vein are brought into view.

In the tissue external to the vein, two kinds of nuclei are shown in the photograph. The first are narrow and elongated, averaging about $\frac{1}{1500}$th of an inch in length and $\frac{1}{9000}$th in breadth. These are the nuclei of the fibre-cells of the muscular coat of the bladder. The fibre-cells themselves are not shown in carmine stainings, but are readily demonstrated in fresh preparations by the action of solutions of osmic acid or of chloride of gold. Indications of the muscular bands formed by the union of these fibre-cells are, however, seen in the photograph, particularly on each side of the principal venous trunk. The second variety of nuclei are oval, about $\frac{1}{3000}$th of an inch long, and belong to the connective tissue of the bladder. The cells in which these nuclei lie are not seen, the action of the carmine being limited to the nuclei. They can, however, readily be demonstrated in fresh preparations by gold-chloride and some other reagents. The cut represents the outlines of a portion of the photograph; *a*, *a*, are the nuclei of the vascular epithelium; *b*, *b*, the nuclei of the muscular fibre-cells; *c*, *c*, those of the connective tissue.

III. Sketch of a photograph representing the stomata between the epithelial cells of a vein $\frac{1}{50}$th of an inch in diameter in the mesentery of the frog. Negative No. 40, New Series. From preparation No. 3276, Microscopical Section. Magnified 400 diameters by Wales's $\frac{1}{8}$th objective. The preparation was made by myself.

Owing to the curved form of the vein, but a small portion of its surface is in focus. In this portion the silver-stained boundaries of several of the epithelial cells of the vein are visible, and display in

their course certain remarkable forms, which may be compared to the Wormian bones of the cranial sutures. These are the so-called stomata. They are irregularly rounded in form, and vary from the $\frac{1}{10000}$th to the $\frac{1}{4000}$th of an inch in long diameter. Those shown in the photograph present a clear central space, bounded by a sharp, black outline, which is sometimes even thicker than the boundaries of the cells themselves. The nuclei of the epithelial cells are not shown. The cut exhibits one of these cells, *a*, with portions of the boundaries of adjacent cells, *b*, *b*, *b*, and the stomata, *c*, *c*, *c*.



VI. Sketch of a photograph representing a minute artery, with part of the adjoining network of capillaries, from the muscular coat of the urinary bladder of the frog. Negative No. 220, New Series. From preparation No. 3378, Microscopical Section. Magnified 400 diameters by Wales's $\frac{1}{8}$th objective. The field is crossed by a small artery, $\frac{1}{1700}$th of an inch in diameter. Its epithelial cells are longer in proportion to their width than those of the veins. They average $\frac{1}{400}$th of an inch in length, and have nuclei similar to

those of the venous epithelium. Wherever the capillaries come into focus the epithelium of their walls is also plainly shown. In the intervascular spaces the nuclei of the muscle and connective tissue appear as in the first photograph. The cut presents an outline of a

part of the picture; *a*, *a*, are the nuclei of the vascular epithelium; *b*, *b*, those of the muscle; *c*, *c*, those of the connective tissue.

The foregoing description of individual photographs will serve to give a correct idea of the epithelium lining the small arteries, veins, and capillaries, as shown in a considerable number of preparations preserved in the Museum, and as observed by me many times in tissues extemporaneously prepared. Both Balogh and Feltz would seem to have been singularly unfortunate in their silver stainings, for they describe the appearances produced as irregular and contradictory. Balogh explains the black lines he occasionally saw in the vessels after silver injections as due to the precipitate of silver occurring preferably on folds in the lining membrane, caused by the irregular shrinkage of the vessel produced by the silver injection, an error readily corrected by combining gelatine with the solution of silver injected; the vessels are thus equably and smoothly distended, yet the epithelium appears mapped out as usual.

Feltz asserts that if a solution of silver be allowed to dry in the light on a collodium film, irregular black lines are produced, quite like those observed after its action on organic membranes. I myself have examined the irregular figures produced by this experiment, and cannot conceive how anyone accustomed to the precise study of organic forms can see any similarity between them and the definite outlines produced by the action of silver solution on epithelial surfaces.

Besides the preparations exhibiting the vascular epithelium which have been described, the Museum possesses, as I have already mentioned, a number in which the epithelium of the skin, of the lymph-sacs of frogs, of the peritoneum and of other surfaces, are mapped out by silver staining, and the reagent is continually employed by myself and my assistants in the investigation of such surfaces. It is impossible for anyone who has had such opportunities for observation, to avoid being struck by the fact that the outlines obtained have a definite form and character for each tissue. It is true that the silver staining does not succeed as frequently as carmine staining does, that its use requires more skill and that failures are more frequent. Sometimes too much action takes place and everything is obscured by the black precipitate produced; sometimes, either because the tissues are not fresh, or the light not sufficient, or from some unexplained reason, the solution does not act at all; but the forms above described as characteristic of the arteries are never observed in the veins, never do the outlines produced on the surface of the skin resemble those seen on the peritoneum, in the lymph-sacs, or in the vessels; each membrane permits only the formation of its own characteristic outlines, never of those belonging to another tissue; moreover, in all cases where it is possible to observe the shape of the epithelial cells without the use of reagents, or to isolate

them, the forms thus ascertained correspond precisely with those mapped out by the silver solution, and when, after the action of silver, carmine staining is resorted to, the nuclei thus made visible correspond in position to the places they ought to occupy, if in fact the silver had mapped out the cell-boundaries, as I certainly believe it does. Whether the discoloration is in the cell-wall, or in the cement or matrix by which the adjacent cells are held together, is a more difficult question, and one into which I do not propose to enter at the present time. It is enough for the purposes of this paper that the peripheries of the cells or the substance just external to them, exhibits a much more speedy and intense reaction with the nitrate than the cell contents do, and must therefore differ more or less from these in composition.

Having arrived at this conclusion with regard to the general interpretation of the action of silver on epithelial surfaces, the question of the true meaning of the so-called stomata next demands consideration. They are to be observed most abundantly in veins of moderate size. I have found them largest and most numerous in veins $\frac{1}{50}$th of an inch in diameter or even larger, and they become smaller and rarer in smaller branches. They are comparatively infrequent in the capillaries and still more so in the small arteries; the Museum however possesses preparations showing them in both. I have moreover concluded from my own observations that in number and size they vary in vessels of the same dimensions in different parts of the body. Thus, for example, in the veins of the mesentery of the frog they are larger and more abundant than in veins of the like dimensions in the urinary bladder of the same animal.

In figure they are rounded, oval, or oblong. I have measured them as large as $\frac{1}{4000}$th of an inch in diameter, but smaller ones $\frac{1}{5000}$th to $\frac{1}{8000}$th of an inch are more common, and the smallest and most frequent do not exceed $\frac{1}{10000}$th of an inch. Sometimes they present clear centres sharply mapped out by black boundaries, sometimes forms of the same size and character are opaque and black throughout, and this has been interpreted as due to variations in the composition of the fluid by which the opening is occupied, which sometimes precipitates the silver solution while at other times it does not, and the action is limited to the solid margins of the pore. They are almost invariably found in the marginal line between adjacent epithelial cells, and the rare cases in which I have observed them apparently in the cells themselves, are probably to be explained by the adjacent margins having from some cause escaped the influence of the silver salt. From my study of these peculiar intercellular forms, I am inclined to regard with favour the opinion that they are actual openings in the epithelial layer.

X. Sketch of a photograph representing a silver staining of the

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external epithelium of the frog's skin. Negative No. 22, New Series. From preparation No. 3036, Microscopical Section. Magnified 400 diameters by Wales's $\frac{1}{8}$th. The preparation was made by myself. The epithelium of this surface consists of a number of layers, and the silver has penetrated in different portions of the skin to various depths. In the photograph the epithelial cells of the upper surface are sharply mapped out, while the boundaries of the cells of several of the deeper layers are seen out of focus beyond. The cells are hexagonal in shape and average $\frac{1}{1300}$th of an inch in diameter. Many of the nuclei have been somewhat tinted by the silver, a circumstance which is not infrequent if the silver action is intense. In the boundaries of the epithelial cells may be seen very many little rings, with black margins and clear centres, averaging $\frac{1}{5000}$th of an inch in diameter, and also many similar forms, of the same size and occupying like positions, which are quite black and opaque throughout. In some parts of the preparation, from which the photograph was taken, almost all the rings are black and opaque, while in other portions almost all present clear centres. The view which regards these rings as true pores certainly appears to me to require fewer suppositions than any other. The cut represents an

outline of a portion of this photograph; *a*, *a*, the nuclei of the epithelial cells; *b*, *b*, the stomata; *c*, *c*, stomata which have become black and opaque throughout. It has been urged, however, by Balogh, that even if the stomata described in the vascular epithelium are admitted as such, they are not large enough to permit the passage of the white blood-corpuscles which, as is well known, average about $\frac{1}{3000}$th of an inch in diameter. But even if we discard the supposition that the pores may be stretched open and made large by the distended condition of the vessels of inflamed parts, there appears to me no difficulty in understanding how a white corpuscle might pass through the smallest of the stomata I have described. An opening $\frac{1}{10000}$th of an inch in diameter is only a little less than one-third the average diameter of the white corpuscles, and anyone who has seen the extraordinary modifications of form which these little masses of protoplasm undergo in the course of their so-called "amœboid movements," would readily credit their capability of passing through such apertures. As the amœboid movement does not occur in the white corpuscles while rolled along in the torrent of the circulation, but only when the movement of the blood is arrested more or less completely, the fact that large numbers of white corpuscles do not habitually pass through the vascular walls into the tissues will not militate against the notion of patulous orifices. That a passage of the white blood corpuscles

through the vascular walls does actually occur, is shown in one of the photographs which we are unable to reproduce.

It will be seen from the foregoing details that, so far as the structure of the vascular walls and the passage of the white corpuscles through them are concerned, the facts appear to be on the side of Cohnheim. How then with regard to the doctrine of inflammation which he builds upon these facts and upon his corneal studies? Does the creeping out of the white corpuscles constitute the essence of the inflammatory process? Do these little movable masses of living protoplasm furnish the germs for the elements of new formations? Have pus-corpuscles no other origin? Are the processes which go on in the cells of the inflamed tissue purely passive, mere phenomena of retrograde metamorphosis?

I find the evidence insufficient as yet to afford satisfactory answers to such questions. The observations made by Cohnheim on the connective-tissue corpuscles of the tongue of the frog are not conclusive in themselves, and Stricker's studies on the same subject* show the necessity of further labour in this direction before the possible multiplication of these elements in inflammation can be denied. As to the doctrine that the white corpuscles, after their escape from the blood-vessels, are transformed into the elements of normal or pathological tissues, the facts hitherto brought forward can scarcely be said to do more than raise it to the rank of an ingenious hypothesis. The actual steps of this transformation, if it does occur, have yet to be observed.

In conclusion I may remark that, as the preparations referred to in this paper form a portion of the Microscopical Collection of the Museum, they can be examined by any professional microscopist who may visit that institution. [The specimens sent with the paper are in Dr. Maddox's possession.]

Army Medical Museum,
Microscopical Section,
July 6th, 1870.

V.—*On the Formation of Microscopic Crystals in Closed Cells.*

By A. W. Wills.

Plate LXII.

I desire to call the attention of microscopists to a mode of obtaining crystallizations of extreme brilliancy, which is, so far as I know, not commonly practised; whereby, moreover, the very act of crystallization may be repeated an indefinite number of times upon the same minute quantity of material, and if necessary exhibited simul-

* 'Studien aus dem Institute für Experimentelle Pathologie in Wien aus dem Jahre, 1869.' Wien, 1870.

taneously to many persons by means of the oxy-hydrogen microscope, either with or without the addition of polariscope effects.

Further, I believe that the facts which I am about briefly to describe deserve attention as having an important bearing on the subject of the modification of crystalline polarity by mechanical force.

The method in question consists in enclosing a very small quantity of a saturated or nearly saturated solution of any given substance at a high temperature in thin cells, and hermetically sealing them before crystallization takes place. It is necessary to describe in some detail the manipulation required to ensure success.

First, then, an ordinary ring of gold-size must be "spun" upon a glass slip, using the size largely diluted with turpentine, by which means a cell is obtained of extreme thinness and perfectly smooth surface. This should then be baked in an oven for some hours, in order to bring it into such a condition that subsequent exposure to a high temperature shall not perceptibly soften it.

Immediately before use, the cell should be placed on the turn-table and the least possible quantity of fresh gold-size applied to it with a brush, the edge of the glass cover being similarly treated. The object of this precaution is to ensure immediate contact between the two without allowing the solution to wet either. The glass slip and cover must be placed in readiness on a hot plate and brought up to a temperature closely approaching the boiling-point of water, and the brass disc of the turn-table heated to a like degree.

The strength of the solution to be used is best ascertained by a few preliminary trials.

A drop of the hot solution is then placed in the cell, the cover adjusted as quickly as possible, the superfluous liquid pressed out and removed with blotting-paper, and the whole transferred instantly to the hot turn-table and hermetically closed by a pretty copious application of gold-size, which does not enter the cell, if the precautions above described have been adopted.

On transferring the slide to the stage of the microscope a most exquisite spectacle is witnessed.

Crystallization rapidly ensues, the crystals extending over the field in forms necessarily of extreme tenuity and corresponding translucency, their transparency being enhanced by the fact of their being bathed in their own mother-liquors; and as a consequence,

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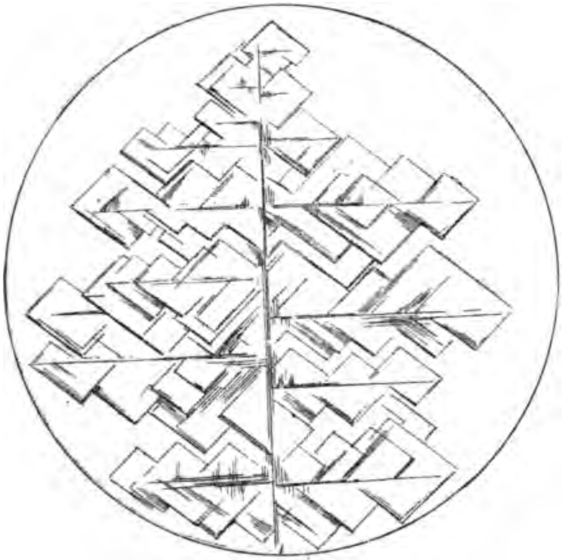
EXPLANATION OF PLATE LXII.

The upper figure represents freshly-formed crystals of chlorate of potash, deposited in a very thin closed cell from a solution saturated at a temperature immediately below its boiling-point—magnified 8 diameters.

The two sides of the lower figure represent the successive phases of re-solution of portions of the same crystals after the lapse of half an hour and twelve hours respectively—magnified 25 diameters.



A.W. Wills del. & lith.

Chlorate of Potash Crystals.

those substances which exert any action on polarized light afford a display of colours truly gorgeous, and far exceeding in brilliancy those obtained in any other way.

By a suitable adaptation of the oxy-hydrogen microscope these effects may be readily exhibited on a disk of 6 or 8 feet diameter.

Now it had occurred to me that from the very fact that the mother-liquors of any given crystallization are necessarily saturated solutions of the substance under examination, no medium should be so incapable of solvent action upon the crystals deposited from them, except, of course, so far as slight changes of temperature might cause on the one hand a trifling re-solution, or on the other a minute increase of their bulk.

I found, however, that a very different result actually ensued.

In the case of nitrate of silver and chlorate of potash, both of which afford crystallizations of extreme beauty both as to form and as to colour under the polariscope, even before the plates have spread to the opposite side of the cell from that at which they have commenced, the edges of the portions first formed lose their sharp definition; dark lines appear parallel to their margins; these become gradually broader, and finally after the lapse of some hours nothing remains but a few shapeless patches.

This result is not more curious than the further observation that the solution can be made with the utmost facility to re-deposit its quantum of crystals: in fact by merely re-heating the slide until the skeletons of the former crystals have been re-dissolved, or even by merely heating for a few seconds to a degree insufficient to produce this re-solution, and then allowing it to cool, the crystallization is reproduced in all its beauty.

I have certain preparations of the two salts above named which scarcely show a trace of solid matter; yet a very slight elevation of temperature suffices to induce so profuse a crystallization, that the entire surface of the cells is again covered by their characteristic forms, which are in turn as completely re-absorbed after a few hours.

Not only is the beauty of these forms very great, but their variety is well-nigh infinite, no two crystallizations being ever precisely alike.

I have operated in this manner upon a large number of substances, organic and inorganic, and although the rate and degree of re-solution varies materially, the same general phenomenon holds good in all cases. It is therefore a matter of interest to inquire to what general law it is due.

It does not appear to have any definite relation to the solubility of the substance, or to the number of equivalents of water of crystallization which it contains, nor again to its relative solubility at different temperatures.

Thus it is displayed in a marked degree by chlorate of potash,

which is an anhydrous salt, requiring 16 parts of water at 60° Fahr. to dissolve it, and only $\frac{1}{10}$th of that proportion at 212°; and equally by biborate of soda, which crystallizes with 10 equivalents of water, and dissolves in 12 parts of water at 60°, and in $\frac{1}{24}$th of that quantity at 212°. Again, by sulphate of magnesia, with 6 equivalents of water, and in which the respective solubilities at 60° and 212° are as 1 to 2; while ferrocyanide of potassium with 3 equivalents of water, and in which the solubilities are also approximately as 1 to 2, exhibits it in a much less degree, the crystals retaining their sharpness for several hours, and even after the lapse of some days undergoing comparatively little degradation.

Now, in crystallizing, the vast majority of substances expand, chloride of ammonium being the only salt *positively* known to occupy more space in solution than in the solid form.

Mr. Sorby * shows that in all cases where, "when they dissolve, the total bulk increases, pressure reduces their solubility; whereas if the bulk decreases, pressure makes them more soluble; in other words, solution or crystallization is impeded by pressure according as mechanical force must be overcome in dissolving or in crystallizing."

I was disposed to seek for an explanation of the phenomena in question by reference to this fact, inasmuch as the effect of crystallization in a closed cell would be, by reason of the increase of bulk in assuming the solid form, to produce a corresponding increase of pressure on the contents; but on the other hand, the contraction of the liquid during cooling must produce an effect in the opposite direction more than sufficient to counterbalance the expansion due to crystallization.

Thus, the contraction of water in descending in temperature from 212° to 32° is about 4·6 per cent., while Mr. Sorby has shown that the increase in solubility of ferrocyanide of potassium due to the pressure of 66 atmospheres is only 1·640 per cent., and would be under the enormous pressure of 100 atmospheres only 2·485 per cent.; while in sulphate of copper (in which the increase of solubility exceeds that of any other substance which he examined) these figures are still only 1·910 and 3·183 respectively. Inasmuch as I have in some instances obtained a re-solution of the crystals first formed amounting probably to 90 per cent. of their total quantity, this cause is evidently quite inadequate to explain the facts.

I am indebted to my friend Mr. George Gore for the suggestion that we must look to the action of capillary force as a cause perhaps adequate to account for this enormous modification of the "force of crystalline polarity."

It is indeed evident that [illegible]itions of the experiment are

* 'Proceedings of [illegible]y,' April 30, 1863.

such as bring this force into active play from the extreme thinness of the films, the enclosing glass surfaces being probably not more than $\frac{1}{300}$th of an inch apart.

I regret that the small amount of time at my command available for such studies has prevented me from pursuing the subject further.

I purpose shortly to endeavour to examine the influence of the depth of graduated cells upon the quantity of crystals of the same and of various substances re-dissolved in given spaces of time, and to investigate the effect of capillary action on the solubility of salts under different conditions on a larger scale.

The result, which I hope to communicate at a future time, will probably afford further confirmation of Mr. Sorby's conclusion that "there is a direct correlation between *mechanical force* and the forces of crystallization and solution."

Birmingham, *September* 10, 1870.

VI.—*The Ciliary Muscle and Crystalline Lens in Man.*

By J. W. Hulke, F.R.C.S., F.R.S.

Part II.

(*Continuation of Aris and Gale Lecture I.*)

Having gained this insight into the apparatus of accommodation as it exists in the human eye, it will not be uninteresting to trace some of its modifications in other members of the vertebrate series.

In all mammalia, monodelphous and didelphous, as far as my observations extend—and through the liberality of the Zoological Society of London, to which I am indebted more than I can express, I have enjoyed unrivalled opportunities of examining the eyes of a large number of animals—the lens and ciliary muscle do not differ in any essential point from those of man.

In mammalia generally the lens is more spherical than in man. In most the central planes are three, as in the human fœtus; in a few (cetaceans and some rodents) there is but one primary plane. The capsule, its epithelium, and the lens-fibres, are essentially like those of man. The ciliary processes are simple, the suspensory ligament and its connections, the arrangement of the ciliary muscle and the kind of muscular tissue are such as we find in the human eye.

Birds, however, present very striking differences. In a bird's eye we are immediately struck with the great extent of the ciliary region compared with that occupied by the retina. It is the stoutest part of the outermost case of the eye-ball, its strength is

increased by a ring of bony plates, and behind those by a cartilaginous lamina intercalated in the fibrous sclera. The ciliary processes are fringed and papillose, not simple, and a plaited membrane, the pecten, projects like a wedge into the vitreous humour from the entrance of the optic nerve.

All the intraocular muscles are composed of striped fibre, the iris as well as those in the ciliary region.

In the iris the muscular fibres are disposed in two sets, one having a radial direction, the other circularly disposed. The radial fibres pass between the great circumference of the iris and the pupil, coursing along the back of the iris just in front of the uveal epithelium. These are the dilators of the pupil. In front of them there lies the stratum of circular fibres, forming a continuous sheet from the pupil to the attached border of the iris, stouter here and at the pupil, and thinner intermediately. They are easily demonstrated in the iris of any large bird by dissecting off the thick layer of pigmented connective tissue which forms the front of the iris, and which is in great part a derivative of the ligamentous tissue which fixes the border of the iris to the margin of the anterior chamber. It is in this connective tissue that the great blood-vessels and nerves lie. These circular muscular bundles which bound the pupil are manifestly a constriction of sphincter pupillæ, but the bundles at the outer border of the iris in contracting not improbably compress the corresponding front of the lens, and so tend to increase the convexity of the uncovered part of the front of the lens in the pupillary area, as H. Müller has suggested. This view of their action derives support from the beautiful points of those bundles which we often find on the lens after death.

The primitive muscular fibres of the iris are much finer than those of the voluntary muscles of the limbs, from which they differ also in dividing and combining in nets.

The ciliary region contains two muscles. In the largest raptorial birds these are quite distinct, they are separated by a considerable interval; but in the eyes of smaller birds these muscles are approximated, and in these their distinctness is less obvious, yet, I think, none the less real.

The foremost muscle was described by Sir P. Crampton, and it bears his name. Behind, it is always attached to the sclera, and in front to the cornea, either directly or to a tendinous prolongation of the inner corneal lamellæ. Shortening of this muscle would therefore tend to bring these attachments together, and as the sclerotic with its bony ring is the least mobile of the two, the muscle would tend to retract or depress the cornea, and so counteract any force simultaneously operating in an opposite direction to increase its convexity.

The posterior muscle always passes between sclera and choroid.

Separated by a wide interval from Crampton's muscle in the eagle, as I have already said, in many smaller birds it appears on a cursory examination to be a continuation of the posterior fibres of this, yet on careful scrutiny always shows that it has distinct attachments. In many birds there is an accessory muscular slip attached anteriorly to a prolongation of the same tendinous band from the cornea into which the inner ends of the fibres of Crampton's muscle are inserted, and in some birds this muscular slip exceeds that which stretches from the sclerotic to the choroid. The contraction of either or of both these muscular slips would tend to drive the choroid forwards upon the sclerotic, and to tighten it upon its contents. They are tensors of the choroid and the homologues of the human ciliary muscle.

The pecten, which before the ciliary muscles were known was once considered as the agent of accommodation, is not any longer considered so. I shall have occasion to refer to it in my next lecture.

Reptiles have an iris very like that of birds. The primitive muscular fibres of the striped sort are extremely fine; they also exhibit divisions and a plexiform arrangement. They are also disposed in two sets, one circular, the other radiating, which differ from those of birds mainly in their minor development. Crampton's muscle I have not found in any reptile's eye, but all which I have examined, embracing several chetonia and many lizards and snakes, have a striped tensor of the choroid passing between the sclerotic and this coat, occupying the same posterior position as its homologue in the bird, and also functionally corresponding with human ciliary muscle.

I have hitherto been baffled in every attempt to decipher the details of the muscular front of the accommodative apparatus in batrachia. The quantity and blackness of the pigment in the frog's eye offers insuperable difficulties. An unstriped sphincter pupillæ is generally demonstrable. It is composed of long spindle-cells enclosing an elongated cylindrical nucleus with some granular pigment. How far it extends outwards, and whether any circular fibres reach the periphery of the iris, as in birds and reptiles, I am unable to say. Radial bundles of spindle-cells resembling those of the sphincter are also certainly present.

Aris and Gale Lecture II.

The conjunctiva is a thin membraniform web of areolar tissue with an external epithelium. The structure of the palpebral part differs slightly from that of the ocular. The former is tougher, the latter has a looser texture. The palpebral part is beset with simple,

vascular papillæ. The epithelium consists of several layers of cells, the deepest of which are oblong and stand vertically, while the more superficial ones are flattened and obliquely packed. The meshes of the areolar tissue often enclose large numbers of lymphoid corpuscles. The blood-vessels and nerves are numerous. The nerves are remarkable for the specialized terminations some of the tubules exhibit—the terminal clubs, end-kolben of Krause. The loose fold of conjunctiva which connects the eye-lids and eye-ball, and also the palpebral front, but more particularly the former, contain small glandiform bodies—minute spherical capsules enclosed in a net of capillary blood-vessels, and according to some observers also surrounded by lymphatics. These when enlarged form the transparent bead-like grains which characterize a kind of granular ophthalmia, fortunately for us much less frequent in Great Britain than the papillose form of this complaint.

The sclerotic in conjunction with the cornea forms the strong outer case of the eye-ball which supports and protects the delicate inner coats. It is thickest behind around the optic nerve, becomes thinner from the iris to the attachment of the tendons of the muscular web, in front of which its thickness again increases slightly. A funnel-shaped canal pierces it behind for the passage of the optic nerve, and it is perforated by many smaller apertures for the transmission of the blood-vessels and of the ciliary nerves. Of these minor openings the only ones which require notice are those by which the venæ vorticosæ choroidæ leave the eye-lid. These pierce the sclerotic obliquely, which renders them valvular, a mechanism that lessens the available opening whenever the pressure on the internal surface of the sclerotic rises unduly, and proportionately retards the egress of the venous blood and raises the pressure still higher.

The sclerotic is principally composed of white fibrous or common connective tissue, in the form of flat fibrillated bundles closely interwoven in planes, which cross one another at every possible angle, but which have a general direction parallel to the surfaces of the coat. Amongst the fibrillated tissue are imbedded simple fusiform and branched corpuscles, which are more numerous in young than in fully-grown persons. The blood-vessels of the sclerotic are not very numerous; the most important ones are, as Leben has pointed out, the recurrent branches of the posterior ciliary arteries, which unite in a small circle which communicates with the vessels of the optic nerve. It is yet uncertain whether the sclerotic has any nerves distributed to it, the ciliary nerves all appear to pass through the coat, and not to furnish branches within their canals.

In birds, lizards, and turtles the fibrous sclerotic is strengthened by the addition of bone and cartilage. The bone is chiefly present in the ciliary region, where it forms the well-known circle

of plates. The osseous tissue contains well-developed lacunæ, and where the plates are thick they are hollowed by vascular canals, and by medullary spaces enclosing fat-cells. It appears to be evolved out of fibrous tissue.

The cartilage is of the hyaline variety. In the track of the sclerotic in many eyes the cartilaginous tissue exceeds the common connective tissue.

The optic nerve pierces the sclerotic a little below and at the inner side of the posterior pole of the eye-ball, the nerve appearing at its inner surface, nearly 1‴ to the nasal side of the fovea centralis retinæ, in the form of a disc, usually circular, sometimes elliptical, and when so the major axis is generally vertical.

The common aperture in sclerotic and choroid through which the nerve passes is a canal narrower anteriorly, where it lightly clasps the nerve, and wider posteriorly, where it loosely embraces it. Around this opening the choroid and the sclerotic adhere very intimately, their fibrous tissues intermingling here concentrically around the nerve.

Here too the minute recurrent branches of the posterior ciliary arteries distributed to the outer part of the sclerotic effect a slight communication with the capillaries in the nerve-sheaths, and indirectly with those in the nerve itself. Some of these last inosculate with the choroidal blood-vessels in the level of the choroidal opening. Through these collateral channels, where the trunk of the arteria centralis is plugged, a small quantity of blood can enter the retina. This choroidal stroma around the nerve contains the same stellar pigment-cells which occur in it elsewhere. In some eyes, in this situation, these cells are more plentiful and richer in pigment, and in such eyes the connective-tissue corpuscles of the neighbouring sclerotic are also not unfrequently pigmented. This excess of pigment expresses itself in the living eye by an incomplete narrow brown or blackish ring round the optic nerve-disc.

In the plane of the choroid and of the inner third of the sclerotic the nerve-opening is crossed by a fibrous web—the *lamina cribrosa* —which peripherally merges in the connective tissues of these two coats. The anterior surface of this perforated lamina is concave, the posterior convex. In the living eye the lamina reveals itself as a white tendinous spot stippled with minute grey dots, the bundles of nerve-fibres going in its meshes. These details are recognizable in the healthy nerve-disc in a small central area which corresponds to a depression which I shall presently describe, known as the physiological pit. A strongly-defined image of these details of the lamina overstepping this limit, and reaching towards or even touching the edge of the disc, is a sign of atrophy.

The nerve-fibres in the trunk of the nerve, behind the lamina cribrosa, are of the opaque or double-bordered sort, while in front of

PROGRESS OF MICROSCOPICAL SCIENCE.

Performance of Vivisection.—Two papers of considerable importance were read at the British Association by Dr. Brown-Sequard; but they were the more so because they led to the following remarks from Professor Huxley, who is especially and very properly desirous of removing experiment from unnecessary bans:—Professor Huxley said, the great theoretical problem they had now to determine was what effect artificial modifications and external conditions had upon living organisms—whether they produced changes which, being transmitted hereditarily, became the basis of new races. Referring to a resolution which had been brought forward at a former meeting, which endeavoured to pledge the association to abstain from making grants of money to persons engaged in experiments which involved vivisection, he said they had before them that day one of the most experienced physiologists and vivisectors of his day, and he had only to ask the audience to form their own judgment as to whether Dr. Brown-Sequard was likely to inflict one particle of pain upon any creature whatever without having a plain and definite purpose in view. For himself he might say that nothing was more grievous to him than to think of the existence of pain in anything whatever. Neither Dr. Brown-Sequard nor himself was indifferent to pain, and he hoped that in no sense were they cruel. He thought that the gentleman who brought forward the resolution to which he had referred, and the gentleman who supported him, hardly knew what they were dealing with. If his friend Dr. Brown-Sequard would pardon his referring to a matter personal to him, he would remind the meeting that that great experimental physiologist, and that accomplished vivisector, who had, he supposed, performed as many vivisections as any man in the world, some years ago thought it advisable to turn the vast knowledge of the diagnosis of disease which he had obtained by this means into actual practice, and he (Professor Huxley) could assure them, from what he knew, that before long his wonderful mastery over symptoms caused his consulting rooms to be absolutely crowded by human beings suffering under multiform varieties of nervous disorders, who sought at his hands and from his knowledge that relief which they could not obtain elsewhere. The prevention of cruelty to animals, when understood in its proper sense, was as good an object as men could devote themselves to, but when they confounded the brutal violence of the carter or the wife-beater with an experiment carried out by a man of science, gently and for the purpose of relieving misery, the enthusiasts in that cause should change their name, and convert themselves into a society for the promotion of cruelty to mankind. If that question came before the Association again, and he hoped it would, he trusted they would recollect that the order of nature was such that certain kinds of truth were only attainable by experiments upon living animals, and that when they might result to the welfare of thousands and thousands of untold human beings who might other-

wise be suffering unimaginable misery, those experiments were perfectly justifiable.

Is there such a thing as Spontaneous Generation?—This is a very serious question, and one which we fear cannot be answered definitely in the present state of science. Nevertheless Professor Huxley, in his opening address to the British Association at Liverpool, attempted to answer it, and gave a very able and remarkably clear account of the history of the whole process, and of the various efforts that have been made in its support and against it to within the last few years. We cannot, however, see that he has done justice to some who have laboured well in defence of spontaneous generation, or who have at all events lately done much to oppose the view which Professor Huxley has taken. We hope to have further information in time for our next number. Meanwhile we may mention that a paper supporting spontaneous generation was read at Liverpool by Dr. G. W. Child, and that a paper against was read at the same meeting by Mr. Samuelson, but that neither of them dealt fully or fairly with the matter.

A New Mode of Studying Capillary Circulation.—The views of Professor Burdon Sanderson and Herr S. Stricker on this subject were communicated to the British Association at the Liverpool meeting. We regret that no report of the paper was given in the public press, so far as we have been able to see.

The British Association in Edinburgh in 1871.—We are glad to see that Edinburgh has been chosen for the meeting of the Association next year. Sir Roderick Murchison said it was the third time the Association would meet in Edinburgh. He did not believe there was any place in the world in which the sciences to which he was attached —geography and geology—could be so admirably illustrated as in Scotland, and it gave him great satisfaction to think that they would have at Edinburgh a number of eminent men of science. Mr. Cowan, late M.P. for Edinburgh, seconded the motion, which was carried unanimously. On the motion of Lord Houghton, seconded by Mr. Gassiot, it was then unanimously decided to hold the meeting of the Association in 1872 at Brighton. On the motion of Professor Stokes, seconded by Mr. Spottiswoode, Sir William Thompson, F.R.S., London, and professor in the University of Glasgow, was unanimously appointed president-elect. The vice-presidents and other officers having been elected, it was resolved that the next meeting of the Association be held about the middle of August, on such days as the council may agree to fix.

The Structure of Fossil Corals.—A very valuable paper, entitled the "Report of Committee on Fossil Corals," was read at Liverpool. The author, Mr. J. Thomson, exhibited several specimens and slices and sections fully illustrating his views.

The Facts of Succession in relation to a Theory of Continuity.—The Department of Anatomy and Physiology was occupied at the British Association with the paper by Dr. Spencer Cobbold, F.R.S., on this subject. After dealing at some length with the opinions of men like

the Rev. F. O. Morris, Dr. Cobbold, having made a series of remarks on Mr. Darwin's work, said that a true conception of what was or ought to be understood by the expression "equivalences"—botanical, zoological, or geological—lay at the very basis of a correct appreciation of the significance of the records of animal, vegetable, or sedimentary roots distributed throughout all time. Further, he ventured to assert that the grandeur of the formative scheme of Nature, whether testifying to an evolutionary method of production or to a series of creative acts, few or many in number, could only be adequately realized by the naturalist whose powers of allocation and grouping enabled him to grasp the magnitude and infinite import of those relations. Dr. Cobbold said he had insisted upon equivalency for years past. He then proceeded to deal with the facts of succession, and said the earliest organism as regarded time which geology had revealed was the fossil called Eozoon, which belonged to the lowermost division of the animal series. Dr. Cobbold then described the succession of the various known groups, and, glancing at the times of origin and succession of the placental mammals, said the first thing that the record suggested was the rapidity with which the most divergent groups made their appearance. Of course, there was no real basis for an assumption of coeval creation, so to speak. It might be fairly held, on zoological grounds, that we ought not to separate man and monkeys, but retain them as one of the twelve under the ordinal title of primates. He adopted the division of the placental into twelve groups, not from any rigid belief as to their separate equivalences, but because they were not only sufficiently distinctive for all practical purposes, and also formed on the whole perhaps the finest expression of grouping which science could at present afford. After dwelling at great length upon the succession of the various groups, he stated that as regarded the highest of all the placental series he would only say that, as he understood the doctrine, the strictest demand of the development theory did not require, as was too commonly supposed, a lineal descent as between bimana and quadrumana; but it was certainly held that either of these groups, as we now knew them, might have been separately evolved from more generalized primatal types, the intermediary terms being possibly connected by a long antecedent and far more generalized common progenitor. In that connection the most advanced evolutionist must candidly own that the assumedly missing tertiary primatals constituted a great and very natural bar to the complete and popular acceptance of the theory of descent by natural selection. On the other hand, the scientific naturalist, whilst admitting these serious deficiencies, threw into the opposite scale a multitude of considerations, the collective value of which seemed to him to outweigh all the data thrown into the anti-continuity side of the balance. For himself, in conclusion, he said that his necessarily limited application of those data was amply sufficient to enforce upon him the provisional acceptance of any theory of continuity. To his mind, its clear application irresistibly implied that nature, to use an old phrase, was but a series of harmonies—wheel within wheel, there being probably but one wheel differing only from all the wheels of

whose limits it was not possible for them to conceive. However, in the contemplation of the phenomena presented to them within that wheel—or that realm of "orderly mystery," as the President had called it—there was ample room and verge for the display of the highest physiological attributes with which man was endowed. Mr. Wallace, who made some remarks on the paper, said he agreed principally with the author.

Animals Dredged up by the 'Porcupine.'—At the British Association meeting at Liverpool, Dr. Wyville Thompson read a paper "On Some of the Echinoderms discovered in the Expedition of H.M.S. 'Porcupine';" and Dr. W. C. M'Intosh read a "Preliminary Report on certain Annelids dredged in the same Expedition." In the course of the discussion which followed, Mr. Jeffrys expressed the hope that in future greater aid would be given by Government to expeditions and explorations having for their object the improvement of scientific knowledge. Our Government had not done in this way what even the poor country of Norway and Sweden had done, what Australia had done, and what he understood Italy was about to undertake. But he looked forward to more attention being paid in this country to scientific wants and requirements, for the sake of education; for no education could be of any value whatever unless it was based upon correct information.

The Germ Theory of Disease.—Mr. W. Hope, V.C., read a paper at the Liverpool meeting of the British Association which goes far to support the dictum of the suppressibility and cure of cattle plague. Mr. Hope said that at an experimental farm belonging to a company in which he was interested, pecuniarily and scientifically, rinderpest broke out in the summer of 1867 among a herd of 260 or 270 cows. He sent for Professor Brown from the Privy Council, who, after making his inspection, said he had found every symptom of rinderpest except one, and that was one of the later symptoms generally, although not invariably, preceding death, *viz.* ulceration of the mouth. Next the dreaded ulcers appeared, and Professor Brown told him there was no means of cure known to science, that the disease was practically incurable, that in the present instance there was no sober, serious chance of saving a single animal out of the whole herd. At his particular request Professor Brown explained the progress of the disease, and the peculiar difficulties to be encountered. Immediately afterwards he (Mr. Hope) undertook the treatment of one-half of the animals. He got all the quick-lime he could lay his hands on, with which he formed broad roadways all round the sheds, three or four inches in depth, and placed pyramids of it along the pathways in the sheds, and slaked it *in situ*, until all the animals were coughing and choking to an alarming extent. He then obtained the Report of the Royal Commission on the Cattle Plague, and specially studied the experiments made by Mr. Crookes, F.R.S., which chimed in exactly with his own instincts, and his reasoning being logical and scientific he made a disciple of him (Mr. Hope) at once. He therefore telegraphed to Manchester for a barrel of genuine carbolic acid, and

determined upon combining the two treatments for the purpose of guarding against the secondary symptoms, with what he might term the chemical treatment recommended by Mr. Crookes. The result was (continued Mr. Hope) that, "while every single animal that I did not take charge of either died or was slaughtered, I succeeded in saving every single animal that I did take charge of." Mr. Hope also recorded some cases of small-pox among his children by no means so decisive.

Professor Huxley on Pennicilium Torula and Bacterium.—Among the various papers read at the meeting of the British Association was one by Professor Huxley, which we should like to have heard, and of which most imperfect abstracts have been given. It was on the above subject. The author stated that what he wished to do was to give some idea of what he imagined to be the relations of those different organisms, and to call attention to some very remarkable circumstances connected with their mode of life and growth. "Professor Huxley, aided by diagrams drawn upon the blackboard as he proceeded, communicated what were most interesting facts regarding the minute organisms which formed the subject of his address, leaving out allusion to the labours of others in the same field." At the close of the address a discussion followed, in which Professor Balfour and Drs. Sanderson and Bastian took part. Professor Balfour remarked that the address was a most important addition to knowledge on the subject treated. "There was, however, a divergence of opinion between one or two of the speakers and Professor Huxley, which led Mr. Belcher, a gentleman who said he was a learner, to complain of a want of unanimity between scientific men, and to suggest, with regard to the question of spontaneous generation, that they should meet and have experiments which would be indisputable. Professor Huxley regretted with Mr. Belcher that everything could not be settled at once, and said he did not think that the British Association was likely to die out by the speedy accomplishment of the consummation desired by Mr. Belcher. Professor Huxley also, in reply to some statements made, said that in his experience, having raised a fluid to the point of ebullition, he had never—although he had repeated the experiment times out of mind—seen the slightest indication of the movement which he had distinguished as the vital movement. If others had seen it, that of course destroyed the value of what he said, but unless they had discriminated very carefully between the vital movement and the mechanical movement which was continually going on, he should still venture to think that nobody had seen the vital movement in a boiling infusion, opened within two or three days after the boiling had taken place; for the longer delay there might be, the greater were the chances of cracks in the vessel, imperfect fastening." Of course, at present it is impossible to offer our opinion on Professor Huxley's paper. We hope, however, in our next to lay the facts more fully before our readers. We have only been able to obtain a most imperfect report of the proceedings of the Association.

Fermentation and Microscopy.—Dr. Angus Smith, in commenting

on a paper read at the British Association by Mr. Tichborne, F.C.S., made some useful remarks which we trust some of our readers will consider:—Dr. Angus Smith said the action of one decomposing body on another had been clearly explained by Liebig, although very curiously he only expressed what others had in somewhat different language said two centuries before. The matter since Liebig's time had taken another form, and the action of decomposing organic substances had been considered less important than the action of what might be called organized substances. This position Liebig objected to very much, but the researches of other chemists had proved that such was the case. It was very possible that both of the actions might go on in nature, for nature was a very wide field. He did not understand from Mr. Tichborne what kind of fermentation was induced. An infinite number of actions might take place from variety in the dust. We might imagine a cholera germ, if there was such a thing, to produce an analogous cholera fermentation. Why should not such germs in travelling about introduce cholera into particular organic bodies, the inorganic substances in filth first receiving it? A filth spot might be said to take the disease and transfer it to human beings. The most important inquiry now was to find out what quality of fermentation took place, when certain qualities of germ were used. This was the heart and marrow of the question for the future.

Spontaneous Generation Defended.—We are glad to be able to say that at the last day's meeting of the British Association the subject of "Spontaneous Generation" received a defence from Dr. Bastian, F.R.S. We cannot give Dr. Bastian's views as fully as we could wish, but we may do more in this direction in our next number. Professor Huxley, in his inaugural address, referring to these experiments, said, the first reply which suggested itself was the probability that there must be some error about them, because they were performed every day on an enormous scale with quite contrary results—meat, fruits and vegetables, the very materials of the most fermentable and putrescible infusions, being preserved to the extent of thousands of tons every year by a method which was a mere application of Spallanzani's experiment. Did the Professor, Dr. Bastian asked, presume that these preserved meats were free from living organisms? The ordinary method of preserving meat in cans, as practised at Mr. M'Call's establishment in London, was this: Large numbers of the cans containing meat, and having only a small aperture in the top, were placed in a bath containing a solution of chloride of calcium capable of being heated to 263° or 264° Fahr. before it boiled, and they were submitted for more than an hour and a half to a heat of 230° Fahr., corresponding with 110° Cent., a temperature which Pasteur always considered sufficient to destroy any pre-existing life which might be in solution. Afterwards, the tins having been hermetically sealed, the temperature was rapidly raised to 260° Fahr., equivalent to 126° Cent., and this temperature was maintained for half an hour. Mr. M'Call assured him that there was a certain definite percentage of failures in meat so preserved. Some of these were undoubtedly to be explained by defective closure

of the cases, but, on the other hand, there were a certain number of failures where it was possible to find no flaw or defect whatever in the tin. They might remain good for two or three years, and then from some unknown cause they were found to become putrid, which was always known by a bulging out of the ends of the tin, caused by the generation of gas inside. Wishing to ascertain the microscopic condition of meats sold as perfectly good, he procured three tins, one of salmon, one of lobster, and one of Jullien's soup. He opened them, and submitted their contents to microscopic examination, and found in each of the tins a very great number of the minute living things which were met with in solutions, and which he supposed to be capable of coming into existence *de novo*. These living things consisted of minute figure-of-eight particles, *bacteria*-like bodies, and filaments. As far as the filaments were concerned, they made no movement, but there was no reason to suppose they were dead. With regard to the movements exhibited by the figure-of-eight particles, there were certain Brownian movements supposed to be due to certain physical conditions of the fluid itself, and there were other movements which were unmistakably vital, and between these two extremes there were any number of conditions. It was quite possible to meet with *Bacteria* which were really living, but which, nevertheless, showed only languid movements. It was impossible to draw any sharp line of demarcation. They could not say positively when these slow movements existed that the thing was living, but, on the other hand, they had no positive right to infer that they were not living. Then Professor Huxley said,—"If, in the present state of science, the alternative is offered to us, either germs can stand a greater heat than has been supposed, or the molecules of dead matter, for no valid or intelligible reason that is assigned, are able to rearrange themselves into living bodies, exactly such as can be demonstrated to be frequently produced in another way, I cannot understand how choice can be, even for a moment, doubtful." By this, he presumed Professor Huxley meant that if these alternatives were put, he would decidedly elect to believe that the germs could stand the heat to which they had been subjected rather than that it was possible for the living things to have been originated *de novo*. Now, his own experiments, at all events, were so simple in their nature, that it did not seem to him that there was very much room for the possibilities of error suggested by the Professor. Certain flasks containing solutions were taken, in some cases of organic, and in others of inorganic matter; they were hermetically sealed by Professor Frankland, and then they were exposed to a temperature of from 146° to 153° Cent. He supposed that this amount of heat would destroy the germs if they were in the flasks, and that the finding of living things in the solutions afterwards would be evidence that they were produced *de novo*. He examined the flasks day after day with the greatest care, and saw nothing until the expiration of about three weeks, when certain cloud-like flocculi appeared in the solution, and after about three weeks more there appeared on one of the flocculi a minute white speck, which, though small, increased in size day by day; and when the flask was opened at the expiration of sixty-five days, the

whole white mass was found to consist of fungus spores and filaments. In order to ascertain the possible effect of such a heat as the solution had been submitted to on fungus filaments, he took certain fungus filaments and spores from an organic infusion and submitted them to the same test, under precisely the same conditions; and when he examined that fungus mass afterwards he found that it was completely dissolved. There was scarcely a perfect and entire fungus filament to be seen; there was not an entire spore. In the face of that evidence he did think it was very hard that it should be maintained that the fungus he found in the first instance was a fungus which had existed in the solution before. It had never yet been shown that any living thing could continue to live after it had been exposed to a temperature of 11° Cent. (? 110), and it was because he had found living things in infusions which had been submitted to a greater heat that the evidence seemed to him at present—and that was all he said—to be decidedly in favour of the view that the living things he found in those solutions had been evolved *de novo*. Dr. Child said Professor Huxley, and those who thought with him, took their stand on M. Pasteur's experiments, and if those did not hold good the whole of their evidence was swept away. They were on the horns of a dilemma. Either these living things were spontaneously produced, or they could withstand the boiling temperature. If they could withstand the boiling temperature, M. Pasteur's experiments were swept away; and if they could not withstand it, then they must be spontaneously produced. Mr. Eddowes remarked that the contents of the cases of preserved salmon and lobster had never reached the boiling-point. They were not prepared by Mr. M'Call, or by the process which had been described, but were prepared on the Canadian coasts in a very rough way, and never kept for any length of time. He suggested that Dr. Bastian should analyze the contents of a case prepared by Mr. M'Call, and he could let him have one which he had had in his possession for fourteen years, and off the contents of which he was prepared to lunch that morning. Professor Tyndall said Dr. Bastian's experiments—conscientiously, earnestly, and laboriously conducted as they had been—had not produced the slightest effect on his views. Dr. Bastian had raised further barriers, obstacles, and objections which could not be met by any argument that could be brought before that meeting. They must be met by a strict scrutiny of his experiments—by going over the same ground; and he would invoke Dr. Bastian himself, in the interest of the subject, to repeat his experiments, multiply them, and seek for negative causes. If he understood aright, Dr. Bastian prepared his flasks by partly filling them with solutions or infusions. They were, he believed, about half filled. (Dr. Bastian: About that.) Then above the solution there would be air, and this, he understood, was removed by means of an air-pump; but he assured Dr. Bastian that it was perfectly impossible for the air-pump to remove the germs or dust particles, or whatever they might be, with which the air was charged, and he should like to know whether the precaution was taken to turn the flasks upside down whilst they were exposed to the heat, so as to submerge these particles

in the heated liquid, because the germs might be able to sustain a much greater amount of heat in a gaseous form than in a liquid form. Dr. Bastian said the flasks were frequently shaken. Professor Tyndall thought that was not enough. Dr. Bastian would have to repeat his experiments before they would be of any value to anybody who required strict proof. Dr. Bastian said that what he maintains is, so far as our present state of knowledge goes, the evidence is in favour of spontaneous generation. The Chairman said, with reference to all these experiments with heat, it seemed to him that the analogy of albumen, which, after it had been exposed to the action of the digestive juice, was no longer amenable to the action of boiling water, was a great light. It was possible that the limit of endurance of heat by vitalized albumen had not been gained. Peptone was a highly complex organized albumen, and nevertheless it was not acted upon by heat at all. Taking a large view of the whole question, he was rather prejudiced against the theory of spontaneous generation.

The Scientific Exploration of the 'Porcupine.'—The copy of the 'Proceedings of the Royal Society' containing Dr. Carpenter's valuable report on the above, has at length appeared, and from its great length and the many valuable appendices it contains, deserves to be in the possession of every naturalist. It extends over one hundred pages, occupying a whole number of 'Proceedings.' It would be impossible to give anything like a review of this paper, which is of the great length we have mentioned, and we can only hope that our readers will procure the copy of the 'Proceedings'* for themselves. The first part deals with the apparatus, and it shows us that the author has paid attention to even the most adverse criticism of his earlier voyage, and has on this occasion taken care to have all the apparatus constructed upon the most thoroughly scientific principles. It is greatly to be regretted, though, that the electric sounding apparatus was found not to work sufficiently well at sea to enable Dr. Carpenter and his assistants to employ it. The details are singularly minute, and the appended reports by W. Lant Carpenter, B.A., B.Sc., Dr. Frankland, F.R.S., and Mr. David Forbes, F.R.S., though short, are of an excellent character.

NOTES AND MEMORANDA.

The Focal Length of Microscopic Objectives.—Mr. C. R. Cross has ably discussed this subject in an American magazine called the 'Franklin Journal.' He remarks:—" The investigation of which the present article is a summary, was undertaken in order to see if some reliable method of measuring the focal length of microscope objectives could not be found. The importance of such a method will be apparent to all who have had occasion to make use of objectives by different

* No. 121.

makers. The focal length of lenses of the same denomination is subject to so great a variation that comparison of these by means of their assumed focal lengths too often gives no true idea of their relative excellence. For example, if two quarter-inch objectives be compared, and one gives results much superior to that given by the other, we cannot be at all sure that the better lens is not really of shorter focus than its designation would indicate." He presents a table giving "the results of several hundred measurements on various objectives, and suggests that an examination of the table will show that the focal length of the objectives of some makers differs considerably from the length marked upon them. For example, No. 34 marked $\frac{1}{2}$ inch is really a $\frac{1}{3}$rd inch objective; No. 33 marked $\frac{1}{4}$th inch is really a $\frac{1}{5}$th inch; No. 29 marked $\frac{4}{10}$ths inch is really a $\frac{1}{4}$th. Lens No. 14 marked $\frac{1}{4}$th inch is really a $\frac{1}{5}$th inch; but Nos. 13, 15, by the same makers, are correctly designated $\frac{1}{5}$th inch, $\frac{2}{3}$rds inch. Differences of this kind must of necessity lead to a great confusion in comparing objectives with one another. I would therefore suggest that each objective made should be measured before being offered for sale, that this confusion may cease to exist. A convenient arrangement would be to fix a glass scale divided to $\frac{1}{50}$th or $\frac{1}{100}$th inch in the draw-tube, sliding in the tube of the microscope, and measure as I have already described. The draw-tube should be moved till the front of the ruled glass shall be exactly 10 inches from the micrometer used as the object. Or it would be more convenient still to have an apparatus similar to the first form, but arranged with a suitable stage and stand so that it can be set at any desired angle. The distance 10 inches (254 mm.), suggested as a standard, is chosen because it is the normal distance of distinct vision, as well as about the length used by microscopists in actual work."

Pædogenesis in the Stylopidæ.—Professor Von Siebold has discovered, says the 'American Naturalist,' September, that the so-called female of Xenos is in reality a larva, and that it produces its young by germ balls like those of the larva of Cecidomyia (Miastor), which produces larvæ like itself during the winter months, but in summer undergoes the usual transformations of these gall flies. This child-reproduction, in individuals without true ovaries, was aptly termed by Von Baer "Pædogenesis."

Air-tight Specimens.—A writer, who simply signs himself "R. H. W.," writes to the 'American Naturalist' to inquire when shall we cease to suffer from the directions sometimes given to mount dry specimens in a cell of pasteboard or paper, fastening the glass cover down by "a little gum" or "paste"? Of course dust or moisture soon accumulates in the cells, or fungoid vegetation grows until it becomes a beautiful and conspicuous specimen; but in any case the original object is tolerably certain to be marred or ruined. He has not unfrequently seen collections of specimens, by popular makers, which have perished in this manner. Lately he lost in this way a very choice specimen prepared by one of the best European makers, whose work is usually faultless; and still later, having occasion to remount a

group of diatoms which had been bought at a considerable price, he found the thin glass cover supported at its four corners by little pieces of pasteboard, and fastened down by pasting over its edges the handsome paper cover of the slide. He has not yet seen any of Bicknell's beautiful specimens prepared in this slovenly manner, but scarcely any maker seems to be entirely exempt. He knows of no cure for this state of things except for microscopists to refuse to buy any specimens except those mounted in balsam, which are on paper-covered slides. Working microscopists can, and often do, preserve dry objects in cells of paper and pasteboard, an arrangement which is both convenient and economical; but such preparations should always be carefully protected by Brunswick black or some other impervious varnish.

CORRESPONDENCE.

Fibres of the Pregnant Uterus.

To the Editor of the 'Monthly Microscopical Journal,' London.

Caracas (Venezuela), *July* 28, 1870.

Sir,—I beg leave to send you the following answer to Mr. Bengafield's query referring to the fibres of the pregnant uterus.*

There is undoubtedly a misprint in Kölliker's original work,† 0·002—0·003‴ being given as normal length of the cells of the contractile fibres of the uterus. It ought to be 0·02—0·03‴, as may be seen by reading on page 570, line 9. On the other hand, in the English translation of the *passus* in question, as quoted by Mr. Bengafield, an important line of the original text is omitted, which runs thus:—"The former (the enlargement) is so considerable, that the cells of the contractile fibres, instead of a length of 0·02—0·03‴, and width of 0·002‴, as before, attain in the fifth month a length of 0·06—0·12‴, a width of 0·0025—0·006, and even of 0·01‴; in the second half of the sixth month a length of 0·1—0·25‴, a width of 0·004—0·006, and a thickness of 0·002—0·0028‴; so that consequently their length is increased from seven to eleven times, and their width from twice to five times."

Now, an enlargement in length from 0·03 or 0·02‴ to 0·25‴ is certainly equal to being increased from seven to eleven times. As the normal width is increased from 0·002‴ to 0·006 or even 0·01‴ (fifth month), Kölliker should have written from twice to *four* times (instead of *five* times).

I am, dear Sir,

Most truly yours,

A. Ernst.

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* 'Monthly Microscopical Journal,' July, p. 43.
† Fourth edition. Leipzig, 1863. P. 567, last line.

Mr. Royston-Pigott's Views.

To the Editor of the 'Monthly Microscopical Journal.'

Ballinamallard Rectory, Co. Fermanagh,
September 10, 1870.

Sir,—Will you allow me space in your Journal for a few remarks on Dr. Pigott's recent discoveries. I do not refer to the question of the Podura-scale; for as Mr. Wenham has undertaken it, others may be well content to leave it in his hands. The discoveries I allude to are those contained in his two last papers. As he has declared that his motto, optically speaking, is "Onward," and repeatedly protested against the folly of our remaining satisfied with our present attainments instead of pressing onward to "higher things," and as these papers are avowedly offered in illustration of the onward movement, he will not, I am sure, be offended if we examine the worth of what he has himself contributed to this end.

The subject of these papers, he tells us, is a difficult one. The problem is "worthy of a great deal of hard labour;" and the student who attempts to follow him must be prepared for "laborious work at the logarithmic tables," with more about the "analytic calculations," and so forth. There is so much of this kind that a reader of average mathematical nerve advances trembling, expecting every moment to find himself sinking in a quagmire of Definite Integrals, perhaps required to solve an equation or two in Mixed Differences. And what are the problems thus brought in with flourish of trumpet and beat of drum? They are these:—Knowing the refractive index of a medium to calculate the refraction of a ray.—Knowing the refractive index to calculate the angle of total reflexion.—Knowing the angle of refraction to calculate the "deviation."—Knowing that the refractive index between two media in the quotient of their indices, to calculate the quotient.

These are the problems Dr. Pigott has undertaken to solve and "tabulate." One naturally asks, Is he aware that he has been anticipated in this field; that these "problems" are solved, with examples, in the introductory chapters of any manual of optics? Two of them are merely arithmetic exercises in subtraction and division; the other two Dr. Pigott will find in, *e.g.* Haughton's 'Manual' (the first which comes to hand), at pp. 19 and 29, with illustrative examples. The tabulation on which he lays so much stress is simply a collection of such examples, for angles regularly increasing; any one of which a beginner could calculate for himself in a few seconds whenever he might happen to need it; and for failure in which an undergraduate would instantly be "plucked" in any university in the kingdom. Our surprise at this method of advancing the higher optics is not diminished when we come upon the following startling announcement:—"In all these inquiries it will *save a deal of time* to remember that when a ray passes from a rarer into a denser medium it is bent towards the perpendicular or normal at the point of emergence."* The prin-

* P. 137.

ciple certainly ought not to be overlooked; though perhaps Mr. Wenham and others who may be trying to extend our optical resources, may think that in saying the neglect of the principle might (in microscopic phrase) occasion "loss of time," Dr. Pigott is, to say the least, putting it very mildly.

Of Dr. Pigott's mathematical knowledge we may form an estimate from his idea that knowing the ratio which the sine of an angle bears to the sine of another given angle, we cannot find the former angle without having recourse to logarithms. For some of his other errors we ought, perhaps, to hold the printer responsible; but what will the Civil Service examiners say to the following exercise in decimals (and there are many of the same kind "carefully calculated"):—The number 1·336 is to be divided by 1·500, and the answer given is 0·890666; that is to say, Dr. Pigott believes that two expressions true to three decimal places only, can be made to yield a quotient true to *six* places.

The only tangible result from these papers is the suggestion that the greater brilliancy of water lenses is in *one* case due to the fact that more of the pencil of light is lost in the air lens than in the other from *total* reflexion. This many persons may not have observed, though self-evident when once attention is called to it. Mr. Wenham indeed had already* ascribed the superiority in part to the smaller loss of light from reflexions; but I think that he there had only the general case in view. Dr. Pigott's remark applies only when the object is artificially mounted (in balsam or some such medium). When the object is in its natural state, either lens transmits the full pencil; but in this case, too, the immersion lens would lose somewhat less light than the other by ordinary reflexions; and from the manner in which Mr. Wenham expresses himself it seems likely that it was only this general case he had in view in the passage referred to.

I hope that nothing I have said in this letter may seem unduly controversial. It is scarcely possible indeed to criticize such papers without some appearance of this kind. Dr. Pigott has come forward not only as the censor of all existing object-glasses, but also as the possessor of improvements which for some unexplained reason seem still to be kept back as secrets. In such a case the only means left us of guessing at the value of what is kept back is by examining the value of what has been published.

Your obedient servant,

S. Leslie Brakey.

* In the June No., p. 303.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *September* 26, 1870.

The Session will commence on Wednesday, the 12th inst., and meetings held on the second Wednesday in each month, at 8 o'clock P.M., as usual.

On the 12th inst. the following papers will be read by Dr. G. W. Royston-Pigott, M.A.:—

1st. "On Aplanatic Illumination."

2nd. "On Aplanatic Definition, with Optical Illustrations."

Walter W. Reeves.
Assist. Secretary.

Quekett Microscopical Club.†

At the ordinary meeting held at University College, August 27th, 1870, Dr. Lionel S. Beale, F.R.S., President, in the chair, seven new members were elected, four gentlemen were proposed for membership, a number of donations to the library were announced, and 113 slides were presented to the cabinet. A paper was read by Mr. T. C. White "On Papers for the Club," in which he offered a variety of suggestions as to the kind of papers it was most desired to obtain, and urged those members who had hitherto been working at subjects in silence to give to their fellow-members the results of their personal experiences by short practical communications at the meetings. Mr. Curties communicated to the meeting an interesting letter received from Mr. Davis, describing some remarkable eggs of insects which had been found parasitic upon the Black Peacock in the collection of the Zoological Society. The subject was illustrated by some very beautifully-executed drawings, and by a number of photographs presented by Mr. Davis for distribution amongst the members. Mr. M. C. Cooke exhibited to the meeting a photograph of the appearance of a portion of *Pleurosigma angulatum*, magnified 98,000 diameters, as shown at the Bailey Microscopical Club in New York, and of which a description has been read at the preceding meeting. It appeared, however, upon inquiry that the photograph was not taken from the object, but from a plaster representation of it. The Secretary read a letter from Mr. C. H. Sterne, of Liverpool, inviting the co-operation of the members on the occasion of the Soirée of the British Association to

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

† Report furnished by Mr. R. T. Lewis.

be held at St. George's Hall on September 22nd. Votes of thanks were unanimously passed to those gentlemen who had read papers, and the proceedings terminated as usual with a conversazione, at which a number of beautiful and interesting objects were exhibited, and a quantity of unmounted specimens, &c., were presented for distribution amongst the members by Messrs Archer, Coke, and Hainworth. *Degeeria domestica*—found for the third time in Britain, was exhibited alive by Mr. Oxley, and Mr. Curties showed specimens of the eggs of bird parasites both in their wet and dry conditions.

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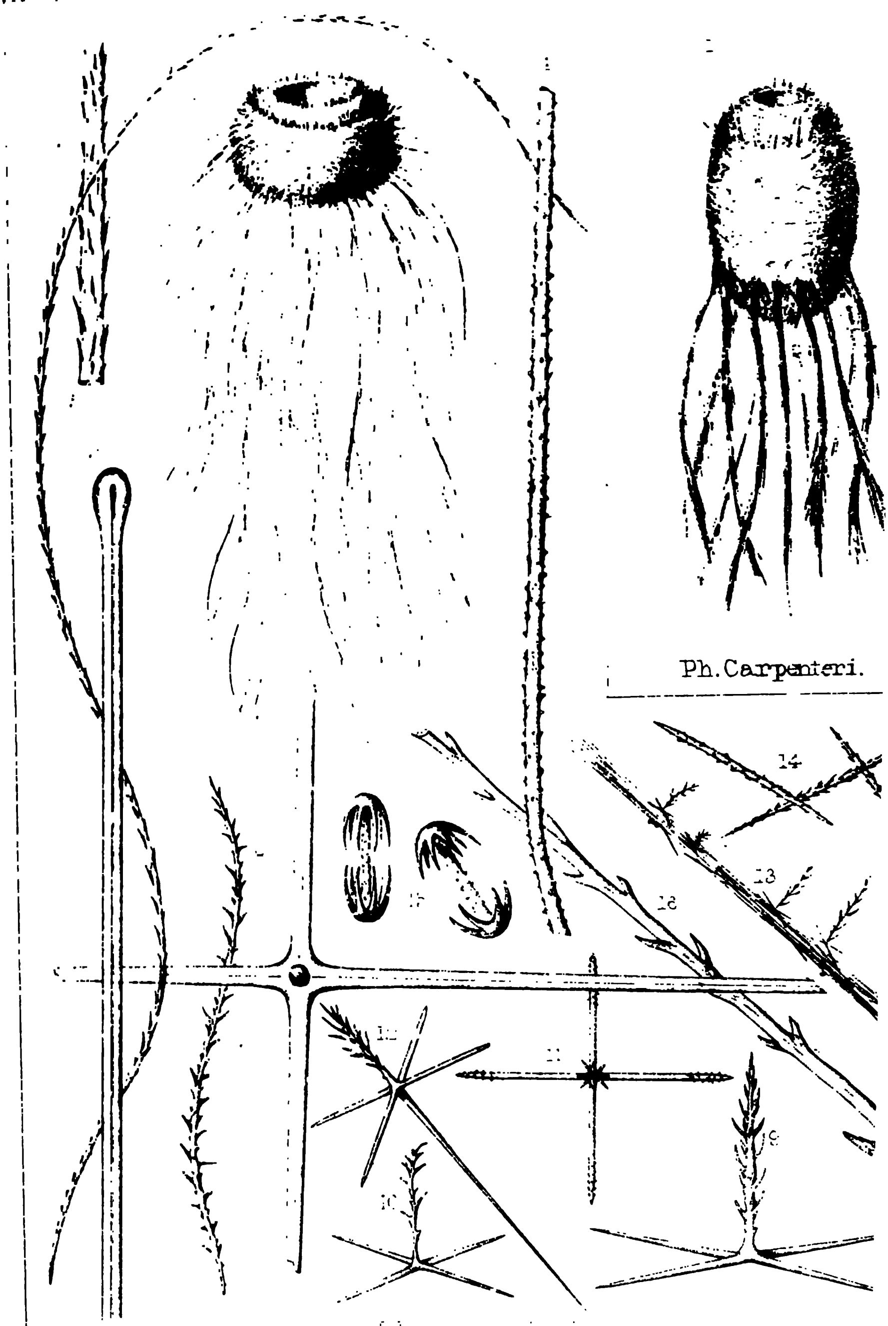
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THE

MONTHLY MICROSCOPICAL JOURNAL.

NOVEMBER 1, 1870.

I.—*On the "HEXACTINELLIDÆ," or Hexradiate Spiculed Silicious Sponges taken in the 'Norna' Expedition off the coast of Spain and Portugal. With Description of New Species, and Revision of the Order.* By W. SAVILLE KENT, F.Z.S., F.R.M.S., of the Geological Department, British Museum.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Oct.* 12, 1870.)

IN the paper it is my privilege to communicate to this Society on the present occasion, I propose to give an account of all those sponges belonging to the same group as the beautiful *Euplectella*,

EXPLANATION OF PLATES.

PLATE LXIII.

FIG. 1.—*Pheronema Carpenteri*, ½ natural size, showing the basal origin and fascicular distribution of the long anchoring spicula.
„ 2.—*Pheronema Grayi*, ⅓ natural size, showing the general and non-fascicular distribution of the anchoring spicula.
„ 3–16 represent spicula from various regions of the same sponge.
„ 3.—Basal portion of an attenuate spinulate spicule from the oscular fringe × 100 linear.
„ 4.—Portion of an attenuate erectly-spined spiculum from the same region × 100 linear.
„ 5.—A filiform adpressly-spined spiculum from amongst the long radiating spicula of the upper region × 100 linear.
„ 6.—A portion of the same still further enlarged.
„ 7.—An irregularly and profusely spined spiculum from the sponge body × 100 linear.
„ 8.—A portion of one of the large attenuate hexradiate spicula from the exterior surface of the sponge body × 100 linear.
„ 9.—One of the "spiculated hexradiate" spicula (Bowb.), having one radius of the perpendicular shaft suppressed, from the interior portion of the sponge × 100 linear.
„ 10.—An irregular variety of the same type.
„ 11.—A spiculum of the same type having the terminations of the lateral radii echinate.
„ 12.—Another variety of the same form, in which the perpendicular radius, suppressed in Figs. 9 and 10, is greatly produced.
„ 13.—Portions of attenuate spicula from the inner portion of the sponge, intercrossing one another, and showing the relation of the spiculated hexradiate and other smaller forms to them × 40 linear.
„ 14.—Minute fusiform echinate spicula from the same region × 200 linear.
„ 15.—Recurvato-birotulate spicula of the sarcode × 400 linear.
„ 16.—Termination of one of the anchorate spicula × 100 linear.

PLATE LXIV.

or Venus' Flower Basket, and the notorious and frequently inverted *Hyalonema*, or Glass-Rope sponge or coral, taken during my recent dredging expedition off the coasts of Spain and Portugal in company with Mr. Marshall-Hall and Mr. Edward Fielding, in the former gentleman's yacht, the 'Norna.'

In the first place, however, I feel myself called upon to acknowledge here my indebtedness to the Council of the Royal Society for their kindness in awarding me a grant of 50*l.* wherewith to defray the cost of the necessary dredging and collecting apparatus, and on which it may be said in great measure the success of the expedition depended. The results accomplished will, I trust, convince the Council of the Royal Society that their confidence has not been misplaced.

Four of the species, belonging to the interesting group just

PLATE LXIV.

FIG. 1.—*Askonema Setubalense*, $\frac{1}{10}$ natural size.
" 2.—Interlacing fascicles of simple attenuate spicula of which the skeleton is composed × 25 linear.
" 3.—A portion of one of these simple attenuate spicula × 100 linear.
" 4.—A variety of the same having a central inflation.
" 5.—Another variety, in which the two extremities are slightly clavate and echinate.
" 6.—One of the larger hexradiate stellate spicula, showing at *a* how the terminations are occasionally spined, × 100 linear.
" 7 and 8.—Two of the smaller hexradiate stellate spicula, having their terminations profusely spined, × 100 linear.
" 9.—One of the multiradiate spicula of the sarcode × 200 linear; at *a* one of terminations of the radii still further enlarged.
" 10.—A fragmentary form discovered among the other spicula, but of uncertain origin, × 100 linear.
" 11.—An ideal restoration of the same.
" 12.—The basal skeleton of *Farrea occa*, in a perfect condition, natural size.
" 13.—The meshwork forming the basal skeleton, viewed from above, × 40 linear.
" 14.—The same viewed obliquely.
" 15.—A furcate spiculated biternate interstitial spiculum (Bowb.), from *F. occa*, × 50 linear.
" 16.—A spiculated biternate variety × 40 linear.
" 17.—A small attenuato-stellate spiculum × 300 linear. Figs. 15, 16, and 17, are copied from Dr. Bowerbank's "Monograph of the Siliceo-fibrous Sponges," 'Proc. Zool. Soc.,' 1869.
" 18.—*Aulodictyon Woodwardi*, natural size, attached to a piece of *Lophohelia prolifera*.
" 19.—A fragment of the reticulated meshwork of the skeleton × 50 linear.
" 20.—A portion of the accessory network composed of coalescing and attenuate hexradiate spicula × 100 linear.
" 21.—One of the attenuate "tension" spicula, having one of the extremities inflated and recurvato-pellate and dentate, × 200 linear.
" 22.—Portion of a variety of the same type, having the pellate expansion replaced by recurved uncini.
" 23.—A free attenuate hexradiate spiculum, with one extremity slightly inflated, × 100 linear.
" 24.—One of the minute "spinulo-quadrifurcate hexradiate stellate" spicula of the sarcode × 200 linear.
" 25.—A termination of a radius of the same, still further enlarged.

PLATE LXV.

cited, out of a total number of nine collected, are altogether new to science, and two of these are described here for the first time. In addition, I am enabled by examination of the material amassed to add considerably to our knowledge of the history and structure of those species already introduced.

Pheronema Grayi, W. S. Kent.

Most conspicuous in the whole series is the fine sponge described under the above name in the 'Annals and Magazine of Natural History' for August last. This beautiful form is known to the fishermen of Setubal as "Ninos de Mer," or "the Sea Bird's-nest;" it bears a strong family likeness to the interesting form dredged up last year by Drs. Carpenter and Wyville Thomson in the Shetland seas, and described by the last-named gentleman in the 'Philosophical Transactions' as *Holtenia Carpenteri*. Unfortunately, the generic name proposed by Dr. Thomson has to give way to that of *Pheronema*; Dr. Leidy, an eminent American naturalist, having previously described as *Pheronema Annæ* a sponge evidently

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PLATE LXV.

FIG. 1.—*Lanuginella pupa*, shown at *a*, natural size, attached to a calyx of *Lophohelia prolifera*.
" 2.—A specimen detached × 12 linear.
" 3.—Interlacing attenuate hexradiate spicula, of which the framework of the skeleton is composed, × 100 linear.
" 4.—An isolated spiculum, with unequally developed lateral radii, similarly magnified.
" 5.—Two of the minute spinulo-multiradiate spicula of the sarcode × 200 linear.
" 6.—A supposed reproductive gemmule × 100 linear.
" 7.—Another and possibly more advanced form, similarly magnified.
" 8.—A fragment of the smooth reticulated fibrous skeleton of *Aphrocallistes Bocagei* × 100 linear.
" 9.—A still more slender and irregularly stellate portion of the same.
" 10.—One of the "spiculated hexradiate stellate" spicula, having the inferior radius of the perpendicular shaft greatly prolonged, × 100 linear.
" 11.—Another variety of the same type.
" 12.—A simple attenuate hexradiate variety × 100 linear.
" 13.—One of the porecto-triradiate spicula of the sarcode × 100 linear.
" 14.—Upper portion of the same, after Oscar Schmidt, and confirmed by my own observations, × 700 linear.
" 15.—A minute attenuate, adpressly-spined, spiculum of the sarcode × 100 linear.
" 16.—A portion of the reticulate and echinate skeleton of *Aphrocallistes Beatrix* × 100 linear.
" 17.—A lateral view of a fragment of the same, showing the spinose prolongations, similarly magnified.
" 18.—One of the porecto-multiradiate spicula × 100 linear.
" 19.—A "verticillately-spined" spiculum of the sarcode × 100 linear.
" 20.—A variety of the same, in which the extremities of the spines are minutely knobbed or spinulate, equally enlarged.
" 21.—An attenuate hexradiate spiculum, with one of the radii spinous, referred by Dr. Bowerbank to this species, but which I have not yet succeeded in detecting, × 150 linear (after Bowb.).

possessing the same generic characters as the one subsequently described in the 'Philosophical Transactions.'*

As a species, my sponge differs from *Pheronema Carpenteri* in many points. In the first place, in the invariably more or less globose contour of its body, and in its large and outwardly expanding oscular orifice. In *Pheronema Carpenteri* the sponge body is usually sub-cylindrical, with an entirely cylindrical osculum. Another distinction is evidenced in the origin and distribution of the long, silvery, hair-like, anchoring spicula, which, spreading out on all sides, serve to fix the sponge in the treacherous ooze from which it was taken. In Dr. Wyville Thomson's species these originate in a number of distinct fascicles springing from the lower portion of the sponge, while in my own they are distributed equally throughout the greater portion of the external surface, without having any definite fascicular arrangement. Dr. J. E. Gray has considered this character sufficient for the institution of a new genus for its reception; but without committing myself to that extent, I regard this feature as an auxiliary and important one in proof of its specific individuality. But in addition to the readily recognized characters of external contour, there are others afforded by the internal spicular structure that assist us in discriminating between this form and *Pheronema Carpenteri*. Having examined the spicules of both species I have failed to find in the last-mentioned one the long attenuate spinulate forms which are present in mine, and also long attenuate erectly-spined ones, which likewise occur in *Pheronema Grayi*. I may also remark that the shafts of the recurvato-birotulate spicula (Amphidisci, W. Th.) are more profusely echinate in *Pheronema Grayi* than in *P. Carpenteri*. This more or less spinous character of the skeletal elements may be quoted as being of very great service in the specific diagnosis of many other forms; as, for instance, between *Aulodictyon fecunda* and another species of the same genus I shall presently introduce.

The specimen exhibited this evening, in company with about a dozen more examples, was procured with the aid of the fishermen's hooks at a depth of, at least, 600 fathoms. On first being brought on board, the sarcode investing and constituting the sponge body was of a brilliant orange colour, a hue remarkably predominant among the Protozoa, while their long, hair-like, anchoring spicula hung about them in matted tresses, cemented together by the

* Since writing my first paper descriptive of *Pheronema Grayi*, I have seen Dr. Leidy's representation of his species ('American Naturalist,' March, 1870). That it is generically identical with the one described by Prof. Wyville Thomson, I have no longer any doubt, and must even consider it to be very closely allied *specifically;* it possesses the same cylindrical outline and the same fascicular arrangement of the anchoring spicula. The smooth upper surface of Dr. Leidy's sponge is evidently owing to its having undergone a considerable amount of wear and tear before it was deposited in his museum. I possess a specimen of *P. Grayi* similarly denuded.

tenacious ooze of the ocean's bed. By careful washing these unkempt locks have been disentangled, and the true origin and distribution of each separate glassy filament been made manifest, while at the same time numerous other delicate organisms have been set free which at first appeared to be inextricably held captive in their mazy embrace. Among these were a quantity of a beautiful transparent Pecten (*P. vitreus*), numerous specimens of a fragile Ophiurid, and a whole host of Foraminifera, which may some day form the subject of a separate communication.

Plate LXIII., Fig. 2, represents *Pheronema Grayi* treated as above described, and Fig. 1, in the right-hand corner, illustrates *Pheronema Carpenteri* under similar conditions. The remainder of the Plate is occupied by figures of the spicula most characteristic of the first-named species.

The sponge I have next to call attention to, though being an indirect result of the expedition, is scarcely less wanting in interest than the form last alluded to.

While engaged in inspecting the numerous treasures contained in the Lisbon Museum of Natural History, my attention was arrested by some dilapidated hat-like bodies, of a whitish-brown colour and felty consistence, which occupied a considerable amount of space on the well-filled shelves. Professor du Bocage, the talented Conservator of the Museum, informed me that Professor Wyville Thomson had recently examined these organisms, and had communicated to him his opinion that they were vegetable and not animal structures. The eminent Professor of Belfast had probably only commenced his sponge studies at that period, otherwise he would hardly have failed to recognize the essentially spongious nature of their tissues. A lighted match, applied by my friend Mr. Fielding, determined on the spot the silicious consistence of the framework of one of the objects of our suspicions, and a subsequent microscopic examination on board the yacht, of a piece kindly placed at my disposal by Prof. Bocage, established beyond doubt the correctness of our premises, while at the same time it revealed to us that we had lit upon a sponge altogether new to science.

We may proceed to summarize its technical peculiarities as follows:—

Askonema, nov. gen.,* W. S. Kent.

Sponge body, bag- or cup- shaped, of felt-like consistence; composed of an interlacement of long filiform silicious fibres or spicula. Interspersed among these, hexradiate spicula of various sizes and minute multiradiate ones with capitate extremities.

[While the revise of this paper passes through my hands, I avail myself of the opportunity of expressing my conviction that "the lovely lace-like vase form, upwards of three feet in diameter at the

* ἀσκός, a bag; νημα, that which is spun.

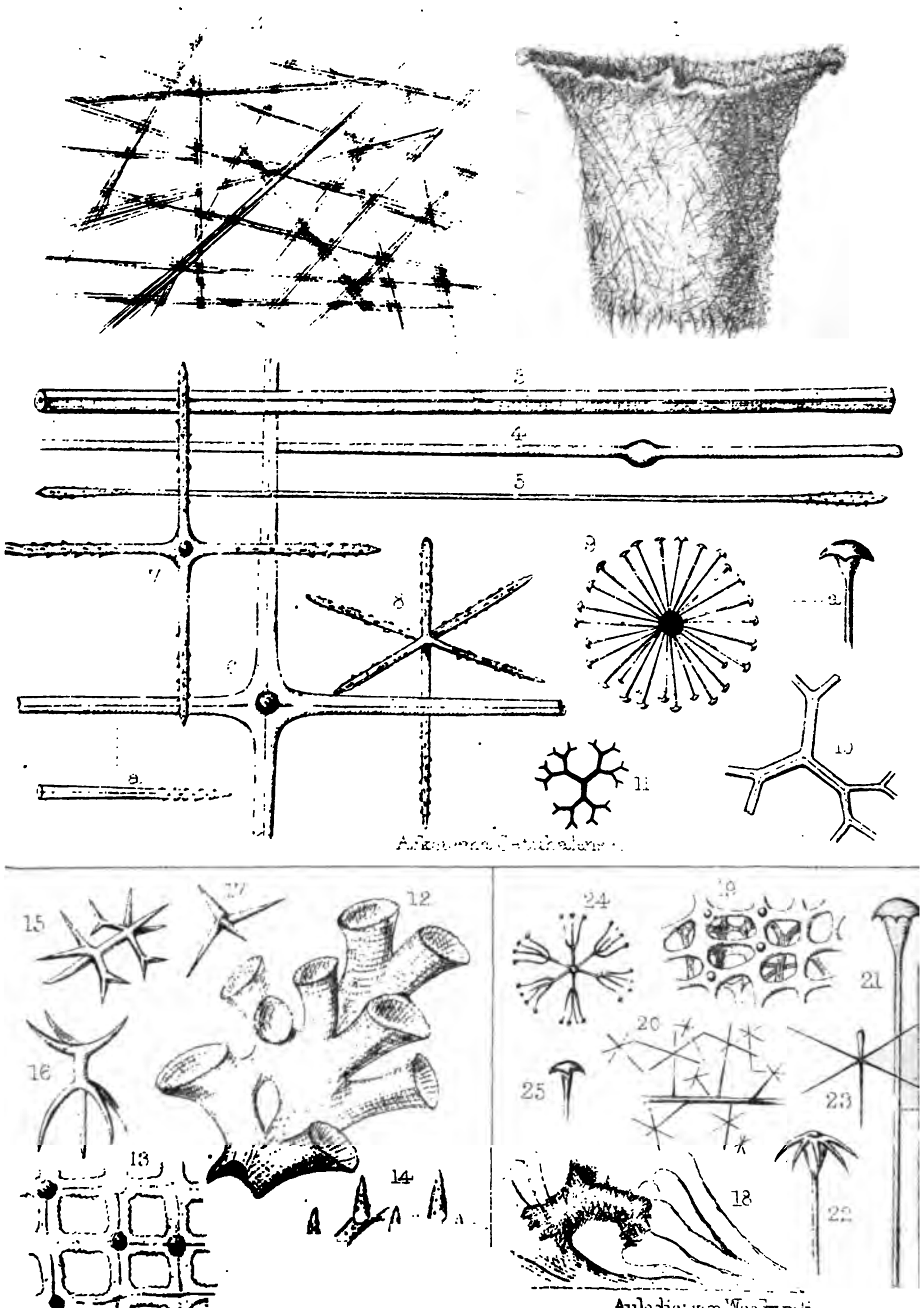
lip," referred to in 'Nature,' for Oct. 20th, as taken in the recent 'Porcupine' expedition, is identical with the species I here introduce under the name of *Askonema*. A few words' conversation with Dr. Wyville Thomson has tended to more fully deepen this conviction. Should my surmises prove correct, my opinion expressed in reference to the habitat of this interesting sponge will likewise be confirmed.]

A. Setubalense, sp. nov.

Sponge body, vase- or sac- like, of large dimensions, expanding superiorly, often upwards of two feet in height; composed of interlacing fasciculi of long filiform fibres or spicula. Individual filiform spicula, smooth, finely canaliculate, varying in diameter from $\frac{1}{6000}$th to $\frac{1}{500}$th of an inch, occasionally possessing a central or excentral inflation, as at Plate LXIV., Fig. 4. Hexradiate spicula of two types, the one large, with smooth attenuate radii, the other of smaller but more varying size, with obtuse extremities and entirely erectly spinous surfaces. Scattered through this sponge there also occasionally occur simple attenuate spicula clavate, and erectly spined at either extremity. (See Plate LXIV., Fig. 5.) Average diameter of the minute multiradiate spicula $\frac{1}{300}$th of an inch.

The radii of the minute sarcode spicula last referred to may originate from a six-rayed foundational portion, as in the "spinulo-multifurcate hexradiate stellate" type of Dr. Bowerbank; but an examination with a high power has failed to eliminate for me such a structure, each individual capitate radius apparently springing from a common irregularly spherical nucleus. All the specimens of this remarkable sponge, which I was accorded the honour of examining, possessed the same ragged basal extremity as represented in the reduced outline given at Fig. 1, so that from its hat-like point of view one would feel inclined to refer it to the species usually supposed to bedeck the head of one of the vivacious inhabitants of the Emerald Isle fresh from the allurements of "Donnybrook Fair," if I may be allowed to make so irrelevant a remark; this feature is, however, replete with scientific significance. It indicates that this sponge was either firmly attached by its base to some fixed submarine body, or otherwise being supported loosely in the muddy ooze, its interior cavity had become filled with sedimentary deposit to such an extent, that the bottom had given way on its removal being effected, either of which suppositions amply accounts for the lacerated inferior margin.

A certain amount of doubt at present remains attached to the exact locality and circumstances in which the specimens of this sponge were taken, some of the fishermen asserting that it is procured from the numerous rocky caverns that fringe the coast-line between Cezimbra and Cape Espichel, while others state that it inhabits the deep-sea fishing-ground, many miles off the coast, in



Askonema Setubalense

Aulodictyon Woodwardi

S. Kent del. Tuffen West

W. West imp.

company with *Pheronema*, *Hyalonema*, and numerous other abyssal forms. The balance of evidence is certainly in favour of the correctness of the latter of these two suppositions, no sponge belonging to the same group having yet been met with within the littoral zone, and the sponges themselves presenting the aspect of having been partially immersed in the same semi-calcareous ooze in which their congeners just referred to are known to occur.

Hyalonema lusitanica, Gray.

One or two specimens of this form were taken, particulars in connection with which I reserve for future publication.

Lanuginella pupa, Os. Schmidt.

This species has only just been described by Dr. Oscar Schmidt in his 'Spongien-Fauna des Atlantischen Gebietes.' The examples examined by Dr. Schmidt were attached to a specimen of *Aphrocallistes Bocagei* in the same manner that those taken by ourselves are attached to a branch of *Lophohelia prolifera*.

The sponge body of *Lanuginella pupa* is cup-shaped, and rarely exceeds one-eighth of an inch in height; the supporting skeleton is composed of a loose interlacement of hexradiate spicula of various sizes, having the appearance under a low power of the microscope of a continuous reticulation, in consequence of the shafts of the spicula being brought in contact with one another through the medium of the investing sarcode.

These hexradiate spicula are of various sizes, with long, slender, smooth, and acutely terminating radii, resembling in miniature the larger form alluded to in the description of *Askonema*; occasionally the basal extremity of the perpendicular shaft is slightly inflated.

Scattered in the sarcode are minute multiradiate spicula with capitate extremities, which must be referred to the "spinulo-multifurcate, or spinulo-trifurcated, or quadrifurcate hexradiate stellate" types of Bowerbank, though at the same time it is a matter of regret we cannot express our meaning in fewer words.

In examining a slide prepared from this sponge I have encountered some minute capsular bodies which appear to partake of the nature of reproductive gemmules, and which will, I think, prove to be the first record of their presence in the particular group of the Calicispongiæ, to which this form is referred in my appended system of classification. Two of these bodies were observed and are figured at Plate LXV.; the smaller one of the two, Fig. 6, presented the appearance of an ovoid, membranous, amber-coloured capsule, having at either pole a radiating fascicle of spicula, which embraced and guarded the extremity of the capsule to which it was attached. The larger, about double the size of the last, exactly resembled it, with the exception that minute simple hexradiate spicula were dispersed

irregularly throughout the interpolar region, seemingly as an additional protection; we may naturally presume that the last-mentioned form is simply a more advanced condition of the first.

Aphrocallistes Bocagei, E. P. Wright.

In the January number of the 'Quarterly Journal of Microscopical Science' for this year, Professor Percival Wright describes and figures a very beautiful reticulated sponge under the name above given, at the same time he is not quite certain of its distinctness from an earlier described species *A. Beatrix* (J. E. Gray), in consequence of the specimens he examined being denuded of the minute and characteristic sarcode spicula. This late expedition has resulted in our obtaining a perfectly fresh specimen of this elegant sponge, and its examination enables me to fully substantiate its specific distinctness from *Aphrocallistes Beatrix*, as also to fill in many links missing in my friend Professor Percival Wright's description. Dr. Oscar Schmidt has likewise recently examined this species with results entirely at union with my own. As Professor Wright remarks, the "areas formed by the reticulated skeleton are much more regularly hexagonal in this species than in *A. Beatrix*, and the "spines (prolongations?) on the bosses" are much more attenuate. But it may be added that the whole skeleton is much more slender, and is wanting in that echinate aspect of the bosses and shafts of the radii, characteristic of *A. Beatrix*. The spicula of the sarcode are also very different, the "porecto multiradiate" spicules are not wanting as Professor Wright imagined, and which in fact appear to constitute the type form of the genus; but there are none of the verticillately spined ones so abundant in *A. Beatrix*; while, on the other hand, *Aphrocallistes Bocagei* is at once recognized by the abundant presence of hexradiate spicula, having one extremity of the shaft profusely spinous and accordingly bearing a close resemblance to those that occur in *Pheronema Grayi*, the opposite extremity of the shaft being frequently very attenuate, as shown at Plate LXV., Fig. 10: the terminations of all the other radii are usually more or less minutely and erectly spined. Dr. Bowerbank figures a spiculum of *A. Beatrix* belonging to the same type,* but the terminations of the radii are quite smooth, and the form is scarce comparatively to what obtains in *Aphrocallistes Bocagei*; this latter feature I remark after careful examination of examples of both species.

Farrea occa, Bowerbank.

A fragment of this species was first figured and alluded to by Professor Owen in his eloquent description of the matchless *Euplectella aspergillum*,† where it is described as the basal mass of some

* 'Proc. Zool. Soc.,' pl. xxii., 1869.
† See 'Trans. Linn. Soc.,' vol. xx., pl. xxi.

unknown silicious sponge. In his Monograph of the British Spongiadæ, Dr. Bowerbank has bestowed upon it the name above given; and has since, in the 'Proceedings of the Zoological Society' for 1869, figured the numerous minute sarcode spicula belonging to it. At the same time the essential basal skeleton of this sponge has hitherto only been obtained in a fragmentary condition, and it affords me much pleasure to give here a representation of its external form in its perfect state. The specimen figured was completely immersed in the hardened mud filling up the interstices of a dead mass of *Lophohelia prolifera* var. *anthophyllites*, and necessarily required some careful manipulation in its extrication. As will be observed, the skeleton of this sponge is composed of a series of infundibular netted tubuli branching out from one another and occasionally coalescing. In the condition in which it was taken, it was almost too much to expect to find the spicula of the sarcode also upon it, and such proved to be the case; but as that portion of its history has already been made known to us by Dr. Bowerbank, this circumstance was of minor importance; and we deemed ourselves only too fortunate to obtain the basal scaffolding intact. The figures of the sarcode spicula accompanying the illustration of this specimen, are copied from those by Dr. Bowerbank, given in his Monograph of the Siliceo-fibrous sponges in the 'Proceedings of the Zoological Society' for 1869.

Aulodictyon Woodwardi, W. S. Kent, nov. gen.* et sp.

From among the branches of the same mass of *Lophohelia prolifera*, which has already been referred to as yielding me so many new forms, I have yet to record another one, which in addition to being a new species must constitute the type of a new genus.

This sponge occurs in small fistulose ramifications, bridging over the minor interspaces between the branches of the coral to which it is attached. It possesses a certain exterior resemblance to *Farrea*, but differs from it in the following particulars:—In *Farrea* the basal skeleton is composed of a single reticulated lamina, in the sarcode investing which, according to Dr. Bowerbank, verticillato-stellate spicula and other minute forms are found. In *Aulodictyon*, on the contrary, the basal skeleton consists of a complex reticulated tube, between and continuous with the primary meshes of which, an abundant network of coalescing simple hexradiate stellate spicula occurs (see Plate LXIV., Fig. 20): the minuter spicula of the sarcode are again of an entirely different type. These are also represented in the Plate. Most remarkable among them are the long attenuate forms, *l.c.* Fig. 21, having one extremity inflated and reflecto-peltate with a dentate margin, and the other attenuately and finely acuminate; these seem to fulfil the part of

* αὐλὸς, a tube; δίκτυον, a net.

tension spicula, and are met with singly or in bundles of two or three together in the substance of the sarcode; occasionally, the dentato-peltate structure is replaced by a simple series of recurved hooks (see Fig. 22), and every gradation between the two varieties may be detected. Another minute and beautiful form of frequent occurrence must be referred to the "spinulo-quadrifurcate hexradiate stellate" type of Dr. Bowerbank: an illustration of this form is given at Fig. 24. Simple attenuate, free, hexradiate stellate spicula are also abundant, these often having the basal extremity of the perpendicular shaft slightly inflated as at Fig. 23. I devote this species to my esteemed friend and colleague, Mr. Henry Woodward.

To this new genus, *Aulodictyon*, must be referred the species figured and described by Dr. Oscar Schmidt as *Farrea fecunda*, the primary characters being essentially in harmony with those applied to the species just described, and only differing in detail. In *Aulodictyon fecunda* (*Farrea*, ditto, O. Sch.) the interspaces formed by the reticulations of the basal skeleton are more regularly quadrate, and of much larger size than in my species; the reticulations are also more distinctly canaliculated, and their "bosses" present the same imbricated or fir-cone like appearance characteristic of *Farrea occa*, while in *A. Woodwardi* the canaliculi are very obscure, and the "bosses" of the reticulations are perfectly smooth. The spicula of the sarcode also differ specifically. I have not succeeded in detecting in my last-named species the attenuate forms with one extremity acutely digitate figured by Dr. Schmidt as characteristic of *Aulodictyon fecunda*, and the minute spinulo hexradiate spicula of his appear to have their terminations trifurcate instead of quadrifurcate as in mine.

Dactylocalyx, Stutchbury.

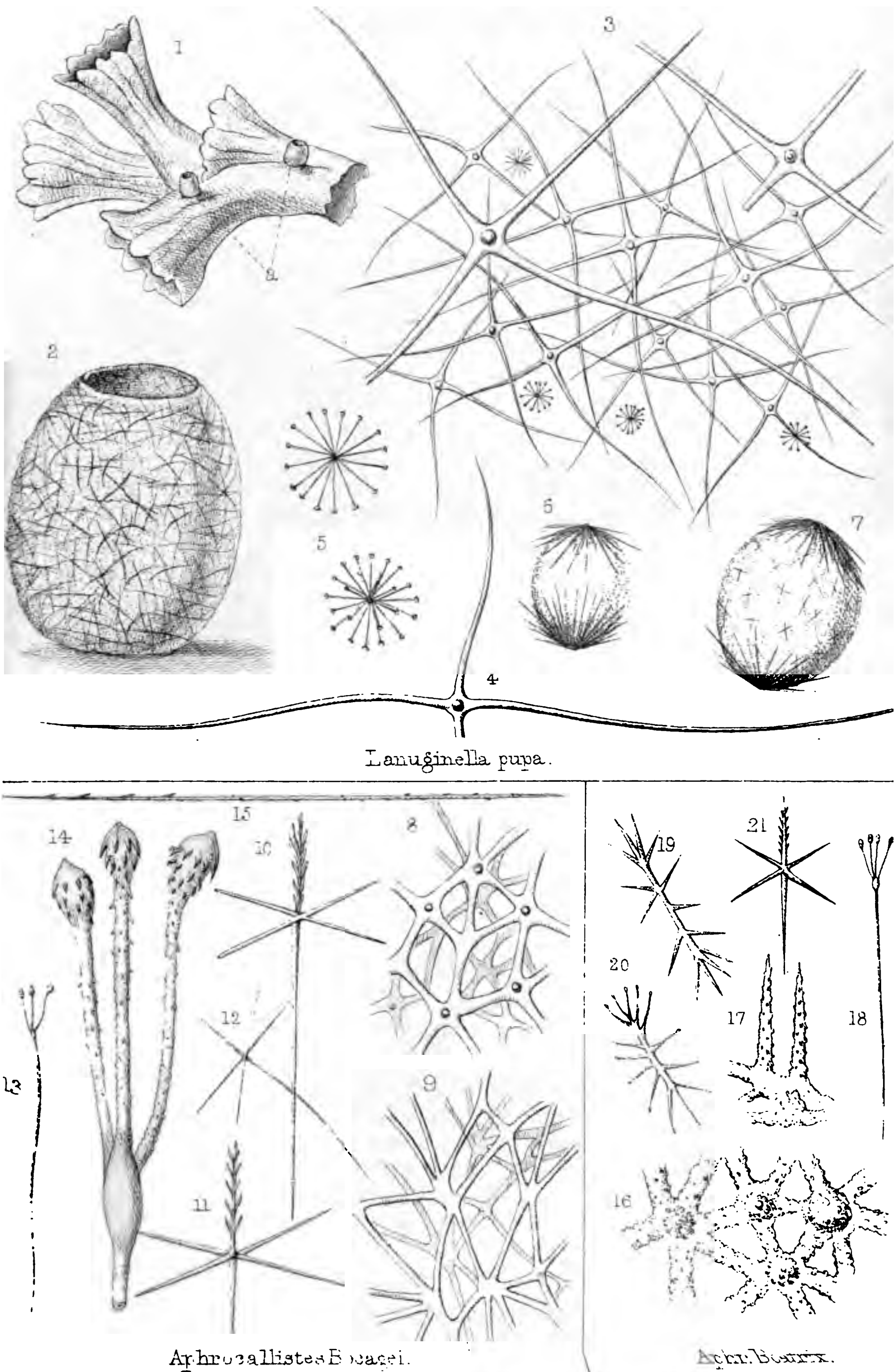
A representative of this genus has been likewise taken, but I have not yet had leisure to determine its specific identity.

Fieldingia lagettoides, W. S. Kent, nov. gen. et sp.

This form is figured and described in the 'Annals and Magazine of Natural History' for September last.

In conclusion, I consider it requisite, in the face of the large series of this interesting group of sponges I have lately been afforded the opportunity of studying, to propose a slight modification of the system of classification which has up to the present time been accepted, and which, in consideration of the limited number of forms known until within a very recent period, was perhaps almost sufficiently significant.

Dr. Gray has proposed to distinguish this group by the name of the CORALLIOSPONGIÆ, its essential character being that the



Lanuginella pupa.

Aphrocallistes Bocagei.

Aphr. Beatrix.

S. Kent del. Tuffen West sc.

W. West

New Sponge-forms.

sponges belonging to it are structurally composed of anastomosing silicious fibres. This feature is evident in *Euplectella*, *Aphrocallistes*, *Dactylocalyx*, &c., but is not applicable to such species as *Hyalonema* and *Pheronema*, or the recently described *Askonema*, *Sympagella*, and *Lanuginella*.

Dr. Wyville Thomson has proposed to distinguish all these forms, taken together, by the name of the VITREA; but the diagnosis he gives is wanting in correctness, and his order must necessarily make room for a more carefully drawn up and more trustworthy one. One of the primary distinctions of his VITREA is,* "that all the spicules of these sponges, without exception, whether of the skeleton or of the sarcode, are referable to the hexradiate type." Such is by no means the case, and is, in short, entirely out of harmony with existing facts. This is fully shown in the simple hair-like spicula forming the greater portion of the skeleton of *Askonema*, in the long anchoring forms and the short fusiform ones of *Pheronema*, and in the various attenuate varieties peculiar to *Aulodictyon Woodwardi* and *fecunda*. Indeed, it is difficult to select any species in which the diagnosis laid down by Dr. Thomson holds good. If the forms just summarized are referable to the hexradiate type, so are the simple acerate ones of *Spongilla*, and the term VITREA may be applied with an equal amount of justice to every other group of the silicious sponges; but it being clearly evident that no such division as the one proposed by Dr. Thomson really exists, we are necessarily driven to seek further for a more carefully and correctly characterized diagnosis.

Dr. Oscar Schmidt, in his fine memoir already quoted, proposes to distinguish the whole series under the title of the HEXACTINELLIDÆ, from the fact of all the species sharing in common the possession of hexradiate spicula; though, at the same time, he does not commit himself, like Dr. Thomson, to the assertion that *every* spiculum is referable to that hexradiate type. This as a primary order or division is so natural, and the name is so fully suggestive of the common character by which Dr. Oscar Schmidt proposes to distinguish the group, that I shall not hesitate henceforth to adopt it, and most naturalists will, I think, recognize its importance and appropriateness.

Subordinate to this primary order, Dr. J. E. Gray's division of the CORALLIOSPONGIÆ, including all those sponge forms with a coalescing or reticulate silicious skeleton, remains intact; while of equal value to this I propose to form a new sub-order, which I distinguish as the CALLICISPONGIÆ, to include all such cup- or sac-shaped forms as *Hyalonema*, *Pheronema*, and *Askonema*, and which, while possessing the hexradiate silicious spicula characteristic of the

* See 'Philosophical Transactions' for 1869, and 'Annals and Mag. Nat. Hist.'

order, are supported by a skeleton composed of interlacing or isolated, but never of coalescing, elements.

I append, arranged under their newly-proposed sub-orders, a synopsis of all the genera at present known, which must be referred to the interesting division of the HEXACTINELLIDÆ.

Order HEXACTINELLIDÆ. Oscar Schmidt.

Sponges with a siliceo-fibrous or siliceo-spicular skeleton. Spicula of the hexradiate-stellate type invariably present.

Sub-Ord. I. CORALLIOSPONGIÆ. J. E. Gray.

Sponge body supported by an anastomosing or continuous reticulate skeleton. Reproductive gemmules entirely membranous, aspiculous (?).*

Gen. *Euplectella*, Owen.	Gen. *Aulodictyon*, W. S. Kent.
Habrodictyon, Wyv. Thomson.	*Macandrewia*, J. E. Gray.
Aphrocallistes, J. E. Gray.	*Dactylocalyx*, Stutchbury.
Farrea, Bowerbank.	*Fieldingia*, W. S. Kent.

Sub-Ord. II. CALLICISPONGIÆ. W. S. Kent.

Sponge body supported by an interlacing or isolated spicular skeleton; never by a reticulate and continuous one. Reproductive gemmules membranous, furnished with protective spicula (?).*

Gen. *Pheronema*, Leidy.	Gen. *Lanuginella*, Oscar Schmidt.
Hyalonema (et *Carteria*), Gray.	*Vazella*, Gray. (*Holtenia* pars, O. Sch.)†
Askonema, W. S. Kent.	
Sympagella, Oscar Schmidt.	

II.—*On a Mode of ascertaining the Structure of the Scales of Thysanuradeæ.* By JOSEPH BECK, F.R.M.S., F.R.A.S.

(*Read before the* ROYAL MICROSCOPICAL SOCIETY, *Oct.* 12, 1870.)

I ENDEAVOURED last year verbally to explain my view of the structure of the scales found on some of the Thysanuradeæ, and having continued my experiments, and having become more than ever convinced of the truth of my views, I think it worth bringing before the notice of the Society in a few words.

I regard the scale as an expansion of the hair, and as designed

* This last character is not laid down yet as essentially diagnostic, though all the evidence so far collected is in favour of it. Dr. Bowerbank records the occurrence of membranous, aspiculous gemmules in the genera *Dactylocalyx* and *Farrea;* while in *Lanuginella,* as observed in my foregoing remarks, forms having the likeness of these bodies, with protective spicula, have been detected. We wait for further evidence of their occurrence in this particular group.

† The generic name of *Vazella* is proposed by Dr. J. E. Gray in substitution for that of *Holtenia* (*H. Pourtalesii,* Oscar Schmidt), which is now necessarily suppressed; but even if Dr. Thomson's genus could hold its ground, a new one would have to be created for the species referred to it by Dr. Schmidt, that form possessing characters entirely different from what obtain in *Holtenia* (*Pheronema*) proper.

doubtless for the purpose of protecting the body of the insect from damp or injury; in order to do both the one and the other, two things are necessary: firstly, that it should be strong; and secondly, that the external surface should be smooth. Both these requirements are fulfilled in those families of this group provided with scales; whether we take *Lepisma*, *Petrobius*, *Tomocerus*, *Templetonia*, *Beckia*, *Seira*, *Lepidocyrtus*, or *Degeeria*, the structure of all is on the same type, although in appearance so different.

The *upper* or external surface is only slightly ribbed as in Lepisma, or mottled or undulated as in the other families, whilst in all the *under* surface is corrugated or strengthened by longitudinal bars or ribs running from the base to the apex of the scale.

Experiment fully bears out this statement, and anyone may try it for himself as follows:—

Place the insect from which the scales are to be obtained on a piece of velvet, and gently press a slip of glass, we will call it No. 1, upon it; the scales will be shed on the under surface of the glass, and the surface adhering to the glass will be the upper or outside surface of the scale. Having obtained a number of scales upon No. 1, place a glass No. 2 upon No. 1, and press them together; some of the scales on No. 1 will adhere to glass No. 2. The surface adhering to glass No. 2 will be the under or inside surface of the scale, or that part next the body of the insect.

Treat both these glasses exactly alike; place each in turn on the stage of the microscope, adjust the object-glass, and breathe gently on the slide. The scales on No. 1 will exhibit a wonderful and most beautiful phenomenon; the moisture from the breath dropping on the scales will run up the furrows in it, and in drying return with the greatest precision, no running across the scale, no irregularity of action, but steadily up and down. The scales on No. 2 glass, on being treated in the same manner, present, on the contrary, a very different appearance; the moisture collects on the exposed surface of the scale in minute globules, and when drying off spreads evenly over the whole surface of the scale, without any apparent direction being given to it by unevenness in the structure of the scale, save an indication of a slightly undulated surface.

But can this experimental appearance be made to coincide with the apparent structure as seen under the microscope? how about notes of interrogation, beaded structure, &c.? I am prepared to assert that it is easily explained, but my present object is to call attention to the mode of ascertaining structure, and to suggest the plan to others, at the same time confidently to assert that the two surfaces of these scales are *dissimilar*, the upper or outer surface being nearly flat, the under or inner being furnished with ribs or bars, varying in number and running from the base to the apex of the scale.

III.—*On the Advancing Aplanatic Power of the Microscope, and New Double-Star and Image Tests.* By G. W. Royston-Pigott, M.A., M.D. Cantab., M.R.C.P., F.C.P.S., F.R.A.S., &c., &c.

Part I.

The continually expanding range of the application of this charming instrument appears to justify the incessant labours now undertaken by optical artists and enthusiastic amateurs for its continual refinement and improvement. Less than half a century ago, a mere toy, it now ranks with the most delicate instruments of physical research; and none appears to have such an array of inventions for heightening even its finest effects.

Yet the whole excellence of the instrument depends upon the veracity of its readings, the truthfulness of its interpretation; that is, the power of the observer to distinguish true from false phenomena, and appreciate, and if possible eliminate, actual sources of error.

At present the lovers of this instrument are somewhat divided in their opinions as to its present merits in the higher departments of microscopical research.

A large majority of observers adhere to the belief in the absolute correction of any appreciable errors; in the perfection, in fact, of our modern instruments.

A writer of acknowledged amateur skill in constructing objectives (the first to form a $\frac{1}{50}$th of an inch focal length) thus expresses his opinions, no doubt pleasantly reflected by numerous vendors of objectives of the first class:—"Objectives from the hands of careful and experienced makers have all been constructed on the globule test, and are not sent forth till every error of workmanship, centering, state of oblique pencils, achromatism, and spherical aberration, are all absolutely corrected; for this discovers the least fault of either, when all others fail."*

And speaking of the immersion system, the same writer tells us—

"Not either the water or glass-cover has introduced a single new element of correction, and will not, therefore, bear out the following assertion in the paper referred to:—'The extraordinary difference between the performance of the hydro-objective and of the pneumo-objective (the plate of air making enormous differences in the aberration of the glasses)† must make it apparent to common sense that our old-fashioned glasses must be wrong somewhere' (the sentence may be completed properly here), 'and if not in failing to converge the image of a point to another point, I know not where to find it, *i.e.* in aberration.'"

* M. M. Journal, p. 302. 1870.

† See Plate LX. on the aberration of the two systems.

It would be difficult to assign a reason for the great advance now made in the performance of objectives since these lines were written,* unless it be in the manner here indicated.

It is a self-evident truism that where no errors exist, correction is unnecessary.

Still, a difficulty arises, which may be thus stated; if the globule test detects the least fault of any optical kind, it seems passing strange that it did not suffice to test inferior glasses up to 1869. Either it failed, or the erroneous indications were ignored or hushed away. If the glasses were absolutely corrected, no further advance was possible. But the advance has been made, the new $\frac{1}{8}$th to witness, therefore improvement was possible; and therefore residuary error remained and may still remain, requiring amelioration or extinction.

The above *statement, as regards aberration,* appears tolerably well fortified by this argument, though it has excited an unusual amount of opposition. A distinguished writer, however, in the 'Student' observes—"Microscopical observers have been very plainly told two startling and unwelcome *truths*, for such we must pronounce them. Firstly, Dr. Pigott says that they have not seen their favourite test-object, the Podura scale (*Lepidocyrtus curvicollis*), properly; secondly, that their best object-glasses are afflicted with sufficient spherical aberration to have rendered the structure what he describes invisible." . . . . .

"So early as 1848, Mr. De la Rue, V.P.R.S., distinguished the beadings of the scale Amathusia Hosfieldii; and he says I have convinced myself (speaking of the cross-stria) by careful and repeated examinations that the striæ themselves are really beaded."

In this paper in the 'Student' a variety of instances are given and figured, in which Lepidopterous scales are distinctly beaded. There is nothing improbable that the same beaded law of structure should take effect in the Podura scale. The only real objection appears to lie in the difficulty experienced by microscopists in defining the beads themselves.

In the same paper Mr. Gosse's 'Evenings with the Microscope' is quoted to this effect:—"The ribs of the Lepisma scale diverge at different angles, those of the upper surfaces being more divergent, divaricating from the foot-stalk, while those of the lower membrane are coarser, more nearly parallel, their bones ranging along the hind edge of the scale. The effect of the intersection of the sets of lines at so acute an angle is to convey the optical impression that the scale is covered with short irregular dashes."

This scale is beautifully figured in Beck's plate with transparent structureless ribs and fine dashes.

I beg again to call attention to my description of the beaded

* May, 1869.

structure of both sets of these striæ. I can see them to the greatest advantage with a power of 800 and a fine half-inch objective; great penetration is required rather than large angular aperture.

I have never yet carefully examined striated scales without developing beaded structure such as—

The *Morpho Menelaus*: *Finest scales of Azure blue.*

Pieris Olearacea.

PODURIDÆ: *Macratoma maritimus.*

Templetonia nitida.

*Podura Transit.** *Macratoma plumbea*, in confirmation of this law.

It is somewhat surprising to me that animadversions have almost entirely been confined to *one* illustration in the "unwelcome" paper; the particulars marked A, B, C, page 302, XII., exciting no examination. But they may be strongly recommended to the attention of microscopists.

But so soon as it shall be ascertained that false appearances supported by "authority" are accepted as the true; that unknown objects are reasoned upon as if they were known; that the creations of the combined errors of the observing instruments, transfigured according to the taste of the partisan, are accepted as perfect transcripts of nature; that, in fact, the unknown is interpreted by a phantasm, a mockery of nature's truth, the philosophy of the microscope will be doubtless degenerating into a dogmatic and ignoble science, falsely so called.

Natural test-objects have in many cases proved delusive in a high degree.

Whenever such objects consist of several complex transparent or rather translucent structures arranged in different planes closely contiguous, false images commingle themselves with the true belonging to a given focal plane, and the resultant image is, under the highest magnifying power, more or less a MIRAGE.

In order to obtain the true image belonging to a given focal plane, the false images must be put as much as possible out of the way—removed or displaced; a desideratum hitherto unknown.

I hope on another occasion to be able to direct the attention of microscopists to some observations in this new field of research.

But to proceed with the subject-matter of this paper, we cannot contradict the fact that the advancing powers of the microscope have been but slowly developed by faltering steps.

It was first discovered that "lined objects" could only be developed by increasing aperture. Scales and diatoms were now constantly examined for their markings and structure. As the objects increased in difficulty, contrivances, ever new, were invented to over-

* Found by Mr. Reade on his transit instrument.

come the miserable definition caused by longer tube and deeper eye-piece.

It soon became a canon, that a third eye-piece was quite high enough to give reliable results; and to such ANDREW ROSS only appealed.

Hence the most strenuous attempts were made to deepen the objective instead of purifying the older objectives of their unorthodox rays continually introduced by larger aperture or greater eccentrical refraction.

At last we have, as it were, awoke out of a pleasant dream to find that the sole reason of the lower glasses such as the $\frac{1}{8}$th and $\frac{1}{5}$th not *carrying* the deep eye-pieces, such as a good telescope bears for instance, are the *errors of the glasses rendered visible* or exaggerated by the high magnifying power of the deepest eye-pieces. That is the very point admitted by all. It requires little logic to reverse the proposition,—That the eye-lenses would not, unless the defects existed in the objectives, give them such unpleasant development as to debar us from using as deep oculars as a good telescope.

Let us recall our experience, and condense it into a sentence—

The better the glass, the higher is the magnifying power it will bear.

The new immersion $\frac{1}{8}$th (Powell's) gives a finer definition with an E eye-piece than old-fashioned glasses of 1869 effected with a B ocular. In other words, the definition is about three times finer than before.

No one doubts that the qualities of telescopes of the same focal length and aperture may be ascertained by the powers they will bear. No reasonable cause can be shown why similar conclusions should not be drawn from applying the same simple method to the microscope.

Before explaining the double-star test it may not be out of place here to allude more in detail to the slow manner in which truth has been forcing itself upon us, and the microscope acquiring fresh powers.

We are indebted to Dr. Wallich for first assigning a pyramidal form to the markings of diatoms. This was a great leap from their mere *lined* character. A glass was once thought excellent which succeeded merely in displaying two sets of intersecting lines! We look back with wonder, now, to our blindness. At that time controversy ran high, as it does now, when each improvement in research is fiercely contested line by line, dot by dot! The *Hippocampus* (*Navicula*) was once a *ne plus ultra*. Long years ago, with honest pride, Topping displayed the double lines of this *Navicula*. In 1841 the black Podura markings with a half-inch were astounding. Ten years later Messrs. Powell exhibited

to me with their new $\frac{1}{8}$th the same black markings transformed into veritable translucent *spines* starting forth in a bold and beautiful solidity of aspect. In 1862, with their best $\frac{1}{8}$th, newly made, of extraordinary aperture, the spherules of Formosum were magnificently plain and decisive.

An inductive argument in general for beaded structure of a transparent nature may be derived from the effects of rotating shadows. I beg to quote the following passage, which involves an important principle:—

"*The Podura beads sometimes exhibit black dots, crescentic shadows, and brilliant points of light.*

"Similar dots may be seen upon the beads of the Pleurosigmata. Had we no visual direct proof of their sphericity, the symmetrical shifting of the crescentic shadows according to the direction of the light would prove their shape."*

This announcement to the Microscopical Society *in May*, 1869, was followed up by confirmatory evidence in the *July following.*

In a paper on the Podura, Mr. Wenham wrote:—"The scale appears marked over its surface with bright blue circular dots, which arise from the thick end of the note of admiration being the best to transmit light, showing that these are real prominences."

It is also stated, "Let a strongly-marked and suitable Podura be now examined with the highest powers, say $\frac{1}{25}$ or $\frac{1}{50}$, using the deepest eye-piece, and even lengthening the draw-tube (and many of our recent objectives will bear this admirably), the illumination being that of the achromatic condenser with adjustable apertures. . . . . Under this excessive amplitude" (about 11,000) "each individual marking retains its characteristic form, and though it is a *body evidently having some bulk*, not the most careful focussing can determine that it stands above the surface of the scale. *The Binocular utterly fails in throwing up the spines in relief as from an underlying surface. . . . . We are unable to prove that they exist in the form of projections.* . . . . If the markings were real spines they would stand out at the line of flexure like short bristles of a piece of hide folded together. But the markings ply round the sharp bend so closely that *the keenest eye cannot detect any appreciable rib or projection.*"

As illustrative of the readings of what are considered indisputably correct glasses, I shall venture to add a further extract from page 125, September, 1870, Journal, from the same writer.

"Some years ago Mr. Ross showed me a *Podura* scale mounted, uncovered, on a black disc, illuminated strictly as an opaque object by a Lieberkuhn, which he had adapted to a $\frac{1}{8}$th of large aperture. The 'note of admiration' markings were finely displayed, and appeared like detached bodies due to surface elevations. This struc-

* Received by the Society M[illegible] and published December 1, 1869.

ture I afterwards confirmed by myself. . . . . The late Richard Beck investigated this test as an opaque uncovered object with a $\frac{1}{8}$th. The light was thrown on the scale by condensing lenses. . . . . 'When they (the markings) are in the same direction as the light, with the narrow ends pointing to it the broad ends appear like brilliant spots, the light from the points is so slight that the scales appear to have lost their markings altogether. Now if the markings were an opaque substance, this result would have been a convincing proof that the markings were depressions; but as we know it to be transparent, it *follows that these particular appearances can only be produced by elevations.*'"

Whilst in July we read, page 27, 1869:—"In all other respects of intervals, form, and position, they (the markings) are the same as under transmitted light, and we are *equally unable to prove that they exist in the form of projections.*"

Amid this discrepancy of projection or no projection, ribs or no ribs, veritable spines, horrific like the bristles of a hide, and dark pigment cells forming vanishing markings, and all melting into beads, "six in one direction and twenty-four in another" in the field of 3 inches and of 12,000 diameters, it is interesting to remark that by two observers (Mr. Reade and Mr. Wenham) beads (subsequent to the announcement) are admitted somewhere. The difference, however, between that and a full resolution is immense. They are described as very sparse in one direction, *whereas the scale is as thickly crowded as can well be conceived.*

In the 'Popular Science Review,' April, 1870, the President writes:—"I can now see with my own powers what *has been before invisible, viz.* the beautifully beaded structure of the whole test-scale as discovered by Dr. Pigott. I found twenty-four beads in $\frac{1}{1000}$th of an inch in horizontal direction, and six in the vertical. This is the utmost I have been able to accomplish . . . . the defect was in the power."

On several occasions I had the honour of exhibiting to the Rev. Mr. Reade, F.R.S., through his Wray $\frac{1}{8}$th, the beads in question as described by him in this passage.

Again, p. 303, "Figs. 4 to 7 in Dr. Pigott's paper do not fairly represent the appearance. The beads are neither so closely packed or so regular as there shown. . . . . The *under* beads may appear to cross either to the right or left, according to the illumination or *fancy* of the observer. Having got the beaded form developed to the best advantage, if we now remove the highest eye-piece and substitute the lowest therefor, close in the tube and adjust the focus (which the change of eye-piece requires), the beaded appearance dissolves into the usual note of admiration markings." (Mr. Wenham.)

Once for all, I must state these are not the general beaded system I have described at all. It should be observed that the

power is here reduced at least *ten* times by changing from the *highest eye-piece and long tube to the lowest* with a closed tube;* and that at the edge of flexure I have frequently noticed a ribbed appearance corresponding to rouleaus of beading on the surface.

The history of our instrument, not without cause for humiliation, presents significant warnings for present and future devotees. In the days of Quekett, a name dear to every English microscopist, we still remember the admiration excited by the grand *chef-d'œuvre* of his plates—the *chequers*, lines and squares of the diatoms so splendidly displayed. The influence of this kind of portraiture, false though it is now known to be, is still felt. Many a costly glass is still in use only capable of producing these elegant illusions. I have seen a $\frac{1}{12}$th from which I could never educe anything but "oblong squares" black and white from the Formosum. The errors to manœuvre this delusion must have been considerable.

Again, when our distinguished President described the "plate of marbles," and the spectator exclaiming, The microscope makes a new start on the Queen's birthday, 1869 . . . . he added, "the terms 'striation and lineation' are no longer admissible . . . . 'striæ and lines' in the books we now have in our hands are just as little applicable to the rows of hemispheres on the surface of a diatom-valve, as they would be to a hayfield with its rows of haycocks. . . . . The markings of these diatoms are described and figured as hexagons . . . . and PHOTOGRAPHY stands by as an attesting witness."

"Even a $\frac{2}{3}$rds inch by Wray, with the D and double D eye-piece, shows the 'plate of marbles' on *P. Formosum*, with abundant light and perfect achromatism."

Our President brought out in this paper, most forcibly, the advantages he had derived from the substitution of parallel rays:— "When they (the lines) reach their proper place, the hemispheres seem to start into existence, and the degree of elevation is conferred *per saltum*. . . . . There they are. I can number them, weigh them, and I can measure them; and number, measure, and weight may be justly represented as the three rectangular co-ordinates of all accurate knowledge of matter." In the next number of the Journal, he says,† "But if, instead of using direct light, we so place an equilateral prism as to throw a parallel beam of oblique light along the length of the (Podura) scale, the shadow of the

* Power of $\frac{1}{25}$ with C eye-piece 2,500
" " long tube 4,000
" " long tube and D D eye-piece .. 11,000
" " short body and low[illegible]e 1,100, or ten times less, nearly.

raised membrane disappears, and we immediately get rid also of the *interior darkness*, and therefore of all trace of 'exclamation' (markings), except that which almost naturally arises at the novel sight of nothing *but small spheres upon what we know to be a scale of Podura*. . . . . These *spherules may often be distinctly* seen on the margin of the scale, and in *more than one instance I have seen them as detached bodies near the scale*."*

A very decided advance is here indicated. A pencil of rays, even though a rough approximation to real parallelism as obtained by a light placed in the focus of a simple plano-convex lens, is far more free from rays of confusion, so hostile to distinct definition, than ordinary condensed light.

In 1862, Messrs. Powell and Lealand constructed for me an apparatus for emitting parallel rays, a description of which was forwarded last year to this Journal; and it was with parallel rays that the beads of a Podura scale were first seen in black dots in the place of the usual notes of admiration.†

The advance of microscopical powers is still going forward. Mr. Wenham in his last communication ‡ has sketched the Podura markings something like an Irishman's "shillalah." Our beautiful notes of admiration are now clubbed at the ends. Dr. Woodward's photographs give also a distinct rounded head to many of them; and Dr. Maddox's sun-pictures scatter beads pretty thickly. It would seem the actinic glasses fail at present to perfect these views: illumination is a vexed question for the photographer, who has never yet succeeded in displaying the spherules of the *Amphipleura pellucida*, which so many English observers have seen microscopically. Unless, therefore, the defining power of photography at least equals the human eye armed with the microscope, no reliable argument can be drawn from its failure or approximate revelations. Indeed the President notices that photography indicated hexagon forms as the correct appearance for the diatom beadings, which we now know is false.

On the other hand, in 1854 Mr. Wenham obtained by photography, photographs of the *Angulatum* magnified 15,000 diameters, and stated that if ever the structure of these difficult tests is to be proved, it will be by the aid of photography.§ He further writes,—"Dr. Pigott, by converting the microscope object-glass into a species of telescope and viewing distant and minute discs of light, professes by means of the 'Aplanatic searcher'!!! (*sic*), to have discovered spherical error in all our best glasses, to the existence of which everyone else has hitherto been blind. Doubtless a very imposing demonstration may be made out of this. . . . ."

If this is really the case, I must consider that Mr. Wenham has paid the highest compliment imaginable. However, he has

* August, 1869. † 1862. ‡ September. § July, 1870.

certainly administered the *coup de grâce* to all researches in this direction by the following final verdict:—"It is most easy to produce the beading (as described *post facto* in July) with the worst of three $\frac{1}{12}$th objectives in his possession, by means of a deep eye-piece and long draw-tube." Mr. Wenham also produces his September knotted cord beading (described by the President and now disclaimed) by his $\frac{1}{25}$th.

These few spherules, such as can be described with the $\frac{1}{50}$th can readily be shown with a fine half-inch and a power of 800, by spoiling the definition of the crowd of beading I have described, *i.e.* by totally disarranging the corrections.

The effect as thus produced may be explained roughly as follows:—When a brilliantly refracting bead is seen in the "clearing" peeping through the single structure, it swells out as all bright points do, as an effect of the error of the glasses, and the false appearance is produced as figured. *Shillalahs* are then rendered instead of *spines* equally false.

The optical difficulties of defining rouleaus of transparent beading, arranged in close parallel lines and crossed by similar rouleaus at a small inclination, cannot be overrated; the *free beads* at the intersections of the lattice-work are of course most readily developed.

In the Degeeria kindly presented to me by Mr. MacIntire, the rouleaus cross at a greater angle than those in *Test-Podura curvicollis*. The upper set may be plainly made out in the smallest and most transparent rounded scales, lying like straightened necklaces of beads or strings of pearls, of a faintly tinted rose colour.

When the adjustments are made for the plane of vision intervening between the two sets, on the large scales both sets may be seen at once. But in all similar cases the beading next the source of light is of a brighter and paler colour than the second set nearer the eye, just as described in D note, December 1869, and as is seen in Lepisma, note C.

Where Mr. Reade and Mr. Wenham have described beads *isolated* and separated by at least six diameters, the beads I have discovered and described a month before these papers appeared, lie *in close* contact, and are at least six times more numerous.

It is to be regretted that large coarse Podura scales of the best sort are exceedingly scarce. It is earnestly to be hoped some enterprising entomologist will rediscover a colony of these interesting insects, which doubtless still exist in abundance.

Mr. Slack, F.G.S., gives another instance of advancing power in the beading into which he has lately resolved the costæ of *Pinnularia major* by means of Powell and Lealand's new immersion $\frac{1}{8}$th, which he eulogizes as a triumph of optical art.

I lately communicated to this gentleman an interesting observation made upon the *P. Formosum*. Each spherule appeared

surrounded with six black round dots; the power was about 4000 diameters, and a similar objective was employed.

In my hands the immersion $\frac{1}{16}$th has displayed the beading of which the median ribs of the

Formosum (*a*)	..	(Diameter, three to one)
Angulatum (*b*)	..	(Diameter, three to two)
and Rhomboides (*c*)	..	" "

appear to be composed. Very curiously the diameter of these beads bore a different proportion to the general beading in each case, as above indicated; a confirmatory fact for the truth of their existence, and negativing the supposition of their being spurious, and guaranteeing their integrity.

The New Double-Star and Image Tests.

Before describing these tests, it will be convenient to lay down an optical principle upon which they entirely depend.

The *smallest section* of any converging pencil of rays is called its *circle of least aberration.*

If a brilliant point of the utmost minuteness be examined with a high-power microscope, it will always present a spurious disk more or less surrounded with coloured rings and diffraction rings. This disk, the best image of a minute point, is always larger than a perfect image. It is this effect which enlarges and confuses minute refracting molecules, which gives a blur to a sharp terminal boundary of a scale, and which renders individual spherules so difficult of perfect definition. It is only with the very finest glasses that these common effects are diminished. A burr envelops the point of light. And lastly, the *spurious disk* is even in good glasses more than double the true size of a perfect aplanatic image.*

The next point to be stated is *that an image is an assemblage of circles of least confusion corresponding to every point in the object.*

It follows from these principles that the slightest deviation from aplanatism or perfect focalization produces an enlarged spurious disk instead of a perfect image of any, the minutest point of light, considered as an object: the object and image being of course placed at what are termed the conjugate foci.

The New Double-Star Test.

Taking Mr. Wenham's further objection as a text:—"In this inquiry it is remarkable how the use of the mercury globule is

* I once heard an optician declare that aplanatic meant a flat field! The perfect meeting of all the converging rays in an absolute mathematical point is of course the proper meaning.

ignored; yet I have no hesitation in saying that without this test it would be impossible to construct perfect objectives." . . . "This test discovers the least fault"—we all admit that a minute globule of mercury, say the $\frac{1}{1000}$th or even the $\frac{1}{5000}$th of an inch diameter, displays a beautiful little image. Its size is easily calculated, which is done at Mr. Wenham's request.

The question now is, Why is the microscope incapable of defining the shape of this reflected image? Two reasons may be assigned—either it is imperfectly defined or formed in the focus of the objective, or else it is too diminutive to be distinguished. The latter can be obviated by using a larger globule; the former arises either from the errors of reflexion from the surface of the mercury, or from errors in the microscope itself.

As for the size, it will be shown further on that for minute globules the image is within a thousand millionth of an inch, the twentieth part of the globular diameter, *provided the object is* 15 *inches from it, and* 3 *inches in diameter.*

Now for the other causes. The image formed by the globule is sufficiently clear of aberration to be distinguished by a perfect glass. The aberration is much more easily calculated than even for a refracting lens, and diminishes as the diameter lessens.

In order to introduce the subject in a clearer manner, I beg to describe an experiment exhibited at the soirée of the Royal Society.

Having observed that plate glass contains air or other gas bubbles of many different sizes and degrees of perfection, and that these lenses possess an aperture of close upon 180°, I found that they form brilliant minute pictures as in a jet-black frame. You can with a Coddington see a whole prospect portrayed within their tiny black rings; the image being formed nearly at the back surface of the bubble.

Placing one of these minute *negative* lenses on the stage, and reflecting the light directly, the flame, brass rim of the mirror, and other details, may be charmingly seen within it, with a low-power objective and a deep eye-piece. But a large-aperture objective and a low-power eye-piece, giving similar power on the whole, fails to give the same picture in its beautiful precision of definition. The instant the aperture is reduced by means of the *aberrameter*, the picture regains its beauty and decision.

This happens with the smallest bubbles, with minute solid *fused* glass spheres, and with minute plano-convex lenses. The effects of this kind of aberration are instantly displayed. If a brilliant flame be imaged in one of these very minute lenses, the shape of the flame is lost; it becomes a round disk, but *much larger than it ought to be.* Detached single spherules from scales and diatoms show the same thing—*spurious disks of refracted points of light.*

Precisely in the same way, the image of a flame reflected by the test globule is a round disk, much larger than it ought to be, as seen in all isolated brilliant points in the microscopical field.

It is interesting then to inquire at what size of image its shape can be discerned. And if such small globules be used that the shape is gone, and if the real diameter of the spurious disk is a test of the correction of the glasses, what becomes of the boasted accuracy of the globule test, in which the size of the spurious disk is totally neglected? No one ever thinks, in testing microscopes, at all about this spurious appearance, or takes any thought about its size, arising from bull's-eye illumination.

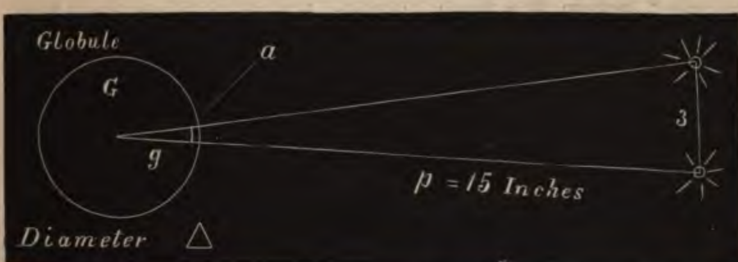
In telescopic testing the most essential part of the ordeal is the diameter of the spurious disk. In a case of optical correction the microscope cannot be exempted from the same *experimentum crucis*. In the diameter of the spurious disk compared with the true image really lies the whole pith of the objective corrections.

But nothing can exceed the coarseness with which the experiment is usually conducted. Thus a bull's-eye condenser, which is known to be one of the worst form for spherical aberration, is placed before a lamp, and discharges a confused pencil of rays upon the test globule, forming a nondescript image upon its surface.

In cases where errors are reckoned by the 100,000ths of an inch a more perfect system is necessary.

I therefore propose the use of Double Stars imaged upon the mercury globule from two brilliant paraffin flames placed edgeways towards the quicksilver for a position of maximum brilliance.

The resolution of the double star must not be considered fair (when the disks are merely ovalized or elongated) except they are really *divorced* or sharply divided. The size of the globule, the distance of the flames and interval separating them being measured, data are at once given for finding the size and separation of the minute disks in the double-star test.



Let G be the globule $\frac{1}{5000}$th of an inch in diameter and $\frac{1}{10000}$th radius; then if the distance of the flames from the globule be *fifteen inches*, and the interval separating their flames be *three* inches,

then the space separating the image flames is almost exactly *one hundred thousandth of an inch.*

If a number of globules be selected of various diameters from the $\frac{1}{100}$th to $\frac{1}{5000}$th of an inch in diameter, the miniature intervals of the same flames undisturbed in position may thus be tabulated:—

DIAMETER OF GLOBULE.		MINIATURE TEST INTERVAL.
$\frac{1}{5000}$	.. ..	$\frac{1}{100000}$
$\frac{1}{1000}$	.. ..	$\frac{1}{20000}$
$\frac{1}{500}$	.. ..	$\frac{1}{10000}$
$\frac{1}{100}$	.. ..	$\frac{1}{2000}$

These results are obtained in the following manner; the flames, 3 inches apart, being 15 inches from the globule,

Let a be the distance between the minute images.
" Δ the diameter of the globule (r its radius).
" q the distance of the images from its centre.

Then optically $\frac{1}{q} = \frac{4}{\Delta} + \frac{1}{15}$ (1) Since $\Delta = 2r$ and $\frac{4}{\Delta} = \frac{2}{r}$.

Also by similar triangles $a : q :: 3 : 15$, or

$$a = \frac{1}{5} q. \qquad (2)$$

To find q, Δ being known by equation (1).

$$a = \frac{1}{5} q = \frac{1}{5} \cdot \frac{1}{\frac{4}{\Delta} + \frac{1}{15}} = 3 \cdot \frac{1}{\frac{60}{\Delta}\left(1 + \frac{\Delta}{60}\right)}$$

$$= \frac{\Delta}{20} \cdot \left\{1 - \left(\frac{\Delta}{60}\right) + \left(\frac{\Delta}{60}\right)^2 - \left(\frac{\Delta}{60}\right)^3 +, \&c.\right\}$$

i.e. $a = \frac{\Delta}{20}$ as the other terms are so exceedingly small as to be neglected.

The interval between the images is $\frac{1}{20}$th of the diameter of the globule,* within the three thousand millionth of an inch when the diameter is $\frac{1}{5000}$.

If therefore microscopists will take the trouble to measure the diameter of the smallest globule which just divides or fairly divorces

* The 2nd term of the series when $\Delta = \frac{1}{5000}$

$$\text{is } \frac{\Delta}{20} \times \frac{\Delta}{60} = \frac{1}{120} \times \frac{1}{500} \times \frac{1}{500} = \frac{1}{120} \times \frac{1}{25000000}$$

$$= \frac{1}{3000000000} \text{ or}$$

the three thousand millionth of an inch. The other terms are smaller still.

the double star, *one-twentieth part of that diameter will be the residuary aberration of the microscope employed.*

It is perfectly clear that no searching for a fine definition among a host of refracting beading packed in one or more strata could ever reveal this simple truth of the double-star test. The superiority of the artificial test by known over natural tests of conjectural structure is here completely demonstrated.

It would not be an honest solution of the difficulty to omit to mention that all single spherical surfaces, whether reflecting or refracting, are afflicted with aberration in its worst form when oblique reflexion or refraction is employed. The image even in the globule is necessarily coarsely defined, infinitesimal as it may seem. A single plano-convex lens, if it could be made infinitesimally small, would still be uncorrected, if spherically formed; as no such single lens can form perfect test images. I have therefore endeavoured to produce images of the utmost perfection by suitable contrivances.

The Image Test.

In a paper read before the Royal Society this year (not yet published) I attempted to explain that images formed in miniature by a finely corrected objective were remarkably free from aberration, the residuary aberration being greatly diminished in the miniature image; and that a fine objective forms in its focus an image of marvellous beauty, when the object is placed at 10, or even 100 inches from it. Double stars formed near the apex of mercury globules, or by means of small given perforations in metal placed before a brilliant source of light, may thus be transferred to the stage of the microscope—avoiding all the *errors* and inconveniences of the minute globule incorrectly illuminated by oblique light.

A variety of interesting objects artificially prepared display in this image all their details when examined under the microscope, with a *perfection* entirely due primarily to the quality of the microscope itself, and secondarily to the quality of the image-forming objective; the apertures of each instrument, the image-viewing and image-forming objective, being regulated to equality.

The correction or examination of the residuary errors in the microscope, under the safe guidance of a perfect test image of a known object, is a new art.

The reality of this kind of test often dispels the illusory performance of "some wonderful quarter" upon some thing of beauty and *a toy* for ever to the admiring possessor. When perfect images cannot be seen, when a brilliant disk appears four times its

proper size, and when some simple objects, such as the glittering leaves of foliage waving in the sunbeam, cannot be reproduced by one objective and are nevertheless beautifully displayed by another —when fog contrasts with clearness; dull, dark portraiture with a bright, sharp, delicate tracing—no guarantee of the great globule-test compensates for such glaring errors when *once* seen and their *modus operandi* understood.

(*To be continued.*)

IV.—*A few Remarks on Dr. Bastian's Papers on Spontaneous Generation.* By Metcalfe Johnson, M.R.C.S.E., Lancaster.*

The question of spontaneous generation has once more been raised by Dr. Bastian in two very elaborate papers in 'Nature,' elucidated by experiments not only fresh but apparently conclusive, if we accept certain postulates respecting vitality in organic matter; but despite the closeness of reasoning, there is yet wanting that something which carries with it such conviction to the minds of Panspermatists as to induce them altogether to abandon their faith and to adopt in its place "such a belief (that living things may and do arise *de novo*) which necessarily carries with it a rejection of M. Pasteur's theory of putrefaction, and the so-called 'Germ theory of disease.'"

Dr. Bastian postulates that a temperature of 150° C. causes death to all organic "germs," and we believe that when the change called "death" has once passed upon a cell (or as Professor Owen says, it is "devived") that it cannot be "revived," but is converted by other living particles to their own sustenance. But what this death or non-life consists in we know not. We see the Polyp growing and we say it "lives," the chalk-stone, and we say it is "dead," but the point of divergence between dead and living we cannot fix.

This difficulty is shown when Prof. Beale attempts to draw the line between "germinal" and "formed" matter, or when H. Spencer essays "the proximate definition of life."

Certain things we know, and certain others we do not know, concerning the power of germs to resist changes of temperature. Thus we know that Vibrions retain their vitality after fifteen days' existence in ice, that confervoid filaments are "revitalized" after having been for some time dried up.† I have myself seen Vibrions active in ten minutes after having been in solid ice for six days.

* It is but fair to Mr. M. Johnson to say that his paper was in our hands before either Professor Huxley's address was delivered or Professor Bastian's paper was read.

† Hassall, Introd., pp. 36, 37.

But we do not know that vitality is destroyed, or that particles developing the phenomena called life can never again *as those particles* "revive" after having been subjected to a temperature of 153° C., for Pouchet says "animalcules and fungi will develop themselves after 200° C. has been applied;" we must therefore "be content to remain in philosophic doubt" for a while longer on the question.

One postulate, it seems to me, must be ceded to Dr. Bastian—that all crystals have ultimate or atomic "germs." What else is the bearing of the atomic theory of Dalton? It presupposes a "unit," and a definite value to that "unit;" otherwise how could we have form? how could an aggregate of amorphous molecules result in a definitely-formed and perpetually-recurrent nature?

The dissepimented pellicle of hay, as shown by Dr. Bastian in Fig. 1, it has not been my good fortune to see, but proligerous pellicles of Vibrions, of Bacteria, and even of Monads, I have often seen, and made drawings of.

But the dissepiments figured by Dr. Bastian cannot be primordial forms, since they contain within themselves the Monads which we believe to be pre-existent.

The transmutation from small circular transparent cells to Amœba I have often observed (see M. M. J., Aug., 1869, Art. VII., p. 104, Obs. June 12).

I am of opinion that the higher powers will show us an amœboid condition of the ultimate Monads themselves. I have also watched the growth of Monas to Uvella, Paramœcium, Kolpoda, &c. (see M. M. J., Aug., 1869, Art. VII., and Jan. 7, 1870, Art. V.). The minute dots in Fig. 3 *a* of Dr. Bastian's paper, June 30, 1870 (and two of the forms in *h*) seem Monads in a state of bilateral segmentation. Fig. 4 *c* I have not seen, but am quite prepared to expect such a diagram from what I have witnessed in the bursting of Pseudo-gonidia (see M. M. J., Aug., 1869, last six lines p. 99, and first four, p. 100) and of Paramœcia (p. 103, ibid., Obs. May 11th).

Aug. 13, 1870.—Saw a Paramœcium burst in a sufficient quantity of water to avoid the suspicion of bursting from drying of the liquid, and discharge itself in granular streams of globular masses; the sarcode remaining shrivelled up, and having the appearance presented by some proligerous pellicle.

In p. 28, Art. V., Jan., 1870, M. M. J., I have referred to a discharge of Euglena similar to the Fig. 5 (p. 174, 'Nature,' June 30, 1870) of Dr. Bastian. Thus far my observation on growth of organisms agrees in the main with that recorded by Dr. Bastian, and refers to the growth from within the primordial utricle, which is essential to the argument of the Panspermatists.

In order to trace as it were the life history of one of Dr. Bastian's *de novo* organisms, we find that in p. 219 he says, "The solution

was submitted to a temperature of 153° C. for four hours." "The plant was completely disorganized; not a single *spore* could be *found*." "No fungus spore has been known to germinate after the fluid containing it has been raised to a temperature of 100° C. for a few seconds." P. 221:—"The starting-point of each alike, *i. e.* the spores and filaments in the crystal, was a mere speck of living matter;" (p. 222) "so extremely minute that" it "only just" comes "within the range of our aided vision." P. 223:— "That such *invisible* germs may have existed in the *form* of colloidal molecules (germs *in posse* but not *in esse*) I am quite disposed to believe;" and, p. 221, "that the spores and filaments (found in the crystal) are the developed representatives of certain specks of living matter, and that these living specks have been evolved by virtue of certain changes and re-arrangements which have taken place among the *non*-living constituents of the crystalline matter." P. 223:—"I have taken spores and filaments from a crystal, and one-half I have boiled for about a minute, whilst the others have not been heated at all. The spores which had been boiled did not germinate, but those which had not been heated soon began to develop filaments. These unmistakable fungus filaments showed dissepiments at intervals, dividing them into chambers, within which were contained large irregular blocks of protoplasm. Occasionally a filament larger than the others might be seen, terminating with a broad convex extremity, and afterwards there gradually appeared on the surface of this the *minutest dot-like projections*, which slowly increased in size and number. The larger of them soon became vesicular, and after a time within the vesicle granules began to cluster so as to constitute a nucleus. I was thus able to trace all the stages of development on one and the same plant, from mere granular abortive-looking Leptothrix threads, only $\frac{1}{20000}''$ in diameter, which gradually grew into a distinct confervoid-looking tube, having broken masses of protoplasm within, into slowly-widening and dissepimented fungus filaments that were capable of producing a head of fructification of the Penicillium type."

A few remarks on these quotations may suggest that although not a single *spore* could be *found*, "many germs of lower organisms are far too minute and too transparent to be seen by the aid of the highest magnifying powers at our command."*

In M. M. J., Jan., 1870, Art. V., pp. 25, 26, I have referred to the early existence of monads as *pin points*, or "*minute dots* only visible by reflected light. Spontaneous movement evident (not currents);" and Dr. Bastian speaks of them as *invisible* germs.

Dr. Hughes Bennett says,† "The molecules out of which animalcules and fungi are produced are not derived from the air, neither can they be supposed to pre-exist in the fluid, as then they

* See Beale, 'Protoplasm,' p. 72. † 'Pop. Sci. Rev.,' p. 62, Jan., 1869.

would be readily seen," but "they (infusoria, vegetable and animal) originate in oleo-albuminous molecules which are formed in organic fluids." It is, however, self-evident that it is useless to expect these sources of "living things" to be revealed to our sight, if, as Dr. Bastian says, they are INVISIBLE, and it is manifest that objects do exist in organic matter much too minute even for our highest powers to identify. Therefore the argument from the heated spores does not necessarily prove devitalization of the germs or *fons et origo* of the spores and filaments.

Dr. Bastian speaks of "the *form* of colloid molecules." The question suggests itself, What is the form or shape of a colloid or an oleo-mucilaginous molecule? Is it vesicular? If so, how does it become so? and, Why not germs?

If the spores and filaments are "*the developed representatives*" of the "living specks," then the latter are the pre-existing source of the former. But the specks arise *de novo*, and their *novum de quo* are the *non-living* constituents of the crystalline matter, which must (like cyanite of ammonia to urea) pass to the colloid state before it can possess *Energia* (p. 172). Dr. Bastian gives us no proof of this change, unless it be in the escape of gas from the crystals. Now an analysis of this gas and the mucedo formed would give a more definite idea whether the C H O N of the tartarate of ammonia was equal to the mucedo, the gas, and the residue. Dr. Bastian says (p. 219), "Germs are supposed by many to be universally diffused in the air and within organic substances." Certain minute transparent cellules, having spontaneous movements similar in appearance to the zoospores which escape from the tubules of Vaucheria from the Pseudo-gonidia of Confervæ, and which I have seen floating freely within the vacuoles of those plants, as well as in the terminal vacuole of some desmids—as Closterium lunula—may be captured from the air by means of an air-sieve (see M. M. J., Aug., 1870).

If we consider the cell-germ as a simple blastoderm (imperceptibly minute), one of a multiple which forms a basement of all organic matter, the osmose of vital dyalysis will explain the phenomena of growth by destruction of other dead matter, the original cell developing the properties of "life." In that case the germs or specks from the tartarate of ammonia (indestructible by known heat) must have been derived from the air, and in the crystals used by Dr. Bastian (old and recent) shreds of cotton and paper fibre were always present. Why should not "germ" or "specks" have been attached to these fibres? Dr. Bastian allows that such germs may be invisible (p. 223).

In observations on the crystallization of tartarate of ammonia, made since reading Dr. Bastian's paper, I have watched the crystalline fibres advance upon the glass and enclose various matters,

such as starch granules and pieces of linen fibre, much larger than the monad forms or "specks," of which there were a great number. I have also grown Leptothrix threads, and watched the crystals bifurcate upon and enclose them. I have also seen the crystals assume a curved direction, without the use of gum, by some apparent obstacle to the straight line of its usual course. But we know of no case in which the phenomena which we designate by the collective term "life" or "living" have been assimilated, and evidence seems to point to the opinion that these phenomena are only developed in a communicable succession, not capable of being generated "de novo." There must be a unit of the primordial Monad, whether as the starting-point of the Leptothrix filament, the "pin point" Monad, or any other early form of development which evinces the phenomena of "life" assimilation and growth within the germinal membrane or primordial utricle,* and reproduction of its like by segmentation, gemmation, or spores; and neither the curved crystal of Rainey, nor the isomeric change of cyanate of ammonia to urea, will afford us any explanation of the development of these phenomena.

There is in the homologous condition of the cells of the more visible unicellular Algæ a foreshadowing of a law of cell development, which seems to extend to all vitalized molecules, from the Monad in the proligerous pellicle to the highest development of brain cells in the human mammal, mainly characterized by bilateral segmentation; this is well seen in the transparent Paramœcium (see M. M. J., Jan., 1870, Art. V., p. 29, Obs. July 25th), in the cells of Chlorococcus, and in the gleocapsoid state exemplifies what may take place all through the series producing double, quadruple, and multilocular cells of 8, 16, 32, &c., going on to form a Pseudogonidium, the original change taking place by binary segmentation of the primordial utricle of each cell, followed by the same changes in each of the segments, and repeated in their sub-segments until a bursting of the sarcode liberates them for individual existence.

Under some circumstances, at present undetermined, the cells early assume an ovoid shape, and in this case the bilateral segmentation produces vacuoles, from the centre of which masses of protoplasm arise. This may be seen in the examination of Palmella cruenta, Oscillatoria, and Lyngbya.

The nucleus theory of development will not account for the division of non-nucleated cells; moreover, there is reason to think many of the so-called nuclei are only contractions of cell wall (see M. M. J., Aug. 1869, Art. VII., p. 103, Obs. April 15, 1868; and Jan., 1870, p. 29. Of Paramœcium, Obs. May 9, 1869, July 25).

Reasoning by analogy, the change may be expected just as much in the living speck or invisible germ as in the perfectly visible cells

* See Beale, 'Protoplasm,' p. 78.

of Chlorococcus, and as such may be expected to fulfil the apparent law of Harvey—

"Omne vivum ab ovo."

There is much evidence for this great law of reproduction, which, commencing in parthenogenesis, passes the successive homologues to the highest condition of copulative generation, with its instincts and powers in a continuous chain.

The theory suggested by Dr. Bastian would appear to be

"Non *omne* vivum ab ovo
Sed *aliquod* vivum de novo;"

and, as shown in p. 223, the successive stages after the birth *de novo* are conducted on the "ab ovo" principle. We must therefore conclude that if an occasional (for the succession *ab ovo* cannot be entirely denied) birth *de novo* takes place, the law "ab ovo" is substituted by a fortuitous concurrence of amorphous colloid molecules. Whereas, if we think with Buffon that each primordial cell is an individual vesicular blastoderm, the primordial Monad may as easily proceed by a continuous succession "ab ovo," as from a colloid arising spontaneously *de novo* from crystalline matter.

A simple answer seems to arise to the proposal of Dr. Bastian, in the note to p. 221, that the specks are mere indifferent matter having *no* inherent tendencies, *viz.* that they have inherent tendencies varying with the stage of progress at which they arrive upon their sphere of independent existence, as mere specks, as zoospores from Mucedinæ, from Confervæ, as Monads, Bacteria, Vibrions, Euglenæ, Amœbæ, or Pseudo-gonidia.

Concerning the spores developed after the thunderstorm referred to p. 222, the ozone produced by the electric discharge will account for speedy growth, which does not take place in ordinary air (see Exp. with ozonized air, M. M. J., Aug., 1869, p. 100, line 5 *et seq.*).

In p. 171 Dr. Bastian says, "The minutest specks of living matter appear in previously homogeneous solutions." Will not the so-called homogeneous solutions, if resolved, like the nebulæ of the astronomer, eventually prove to be an aggregate of Monad forms? On the 4th of August, 1870, I observed some Paramœcia swimming freely in a homogeneous fluid, which with reflected light proved to be one mass of Vibrions.

Considerable difficulty arises as to the agents required to devive organic germs or colloid molecules *in esse*. Dr. Hughes Bennett says, "Neither calcined air, sulphuric acid, liquor potassæ, gun-"cotton, nor a boiling temperature have prevented the production of "infusoria, or destroyed the supposed germs in the air or infusion." I have seen (Aug. 1st, 1870) Vibrions active after sufficient strong

ammonia had been applied to the liquid to arrest instantaneously all movement of Paramœcia, &c.*

In Art. V., M. M. J., Jan., 1870, pp. 25, 26, are the details of an experiment with water passed through a piece of lint exposed to the air, in which monads were first apparent; and, Sept. 17th, monads, some active, others inclining to germ colour; Oct. 8th, a large deposit of green Chlorococcus. The following notes occur subsequently:—

Nov. 1.—Chlorococcus round; many oval; smaller; a few large, with vacuole; a few short tubules, as if soridia. No acuminated cells, as of Oscillatoria.

Dec. 31.—Gonidia in clusters, tending to lines. No filaments.

July 3.—Floating on surface a large number of filaments of Lyngbya, with Chlorococcus—Gonidia.

This bottle was kept closed except at the periods of examination. Care was taken to prevent the introduction of any foreign matter by the instruments used to remove the substance for examination. The conclusion, therefore, is that Lyngbya may be developed from Monas; and the opinion is supported by the following observations; and the argument deduced from it supports Buffon's view, here reproduced, that all organic matter consists of basement cells having more or less hereditary tendency.

But the following experiment shows also that similar forms of vegetal product are evolved from different plants:—

June 4th, 1870.—I put a piece of Jungermania asplenoides, marked A; a few stalks of Polytrichum commune, marked B; a small quantity of Sphagnum palustre, marked C; and a few sprigs of Hypnum rutabulum, marked D, into 2-oz. bottles, filled with water and exposed to light and air. A and D were less vigorous than B and C, and had less root.

June 7th.—No moving life visible.

„ *8th*.—Numerous Monads in B and C. No moving life in A and D.

June 9th.—B, Monads very active; a patch of green oval Gonidia. C, Monads transparent; oval; very active. A and D, no moving life.

June 11th.—One large Pseudo-gonidium from B. C, a large number of transparent Monads; some moving rapidly; others slowly. A and D, no moving life.

June 17th.—A, no moving life. B, green Gonidia, round. C, transparent, globular, moving cells. D, one transparent oval cell.

June 25th.—A, one green Gonidium, still. B, large number of green Gonidia, still. C, round and oval transparent cells, still; a large number of active Monads.

July 3rd.—C, surface covered with green cells, round, of all sizes, to full-sized Chlorococcus; oval cells from small size to medium-

* See also 'Lancet,' Aug. 6, 1870.

sized Lyngbya; a few aggregated round cells, with nuclei. On side of bottle masses of round nucleated Chlorococcus, oval green cells, and larger Lyngbya. B, large oval cells, round nucleated cells, and one green filament with vacuoles, each containing three or four chlorophyll cells. D, a few filaments of Oscillatoria; a very few round and oval green Gonidia.

July 12th.—C, Chlorococcus and Lyngbya. B, round cells; minute green, and full-sized, round, nucleated Chlorococcus; also oval green Gonidia, very active (*queræ*, Euglena?). D, oval still Gonidia and Lyngbya filaments.

July 15th.—C, every size of green Gonidia, from small, smooth cell to gleocapsoid granular; also of oval cells every grade to full-sized Lyngbya.

July 25th.—B, C, and D, all developing Chlorococcus and Lyngbya. A, a large quantity of Monads, some still, mostly active.

Aug. 4th.—A, Chlorococcus abundant. B, Chlorococcus and Lyngbya. C, Chlorococcus and Lyngbya very abundant. D, Chlorococcus and rudimentary Lyngbya, with three vacuoles, each containing a mass of chlorophyll.

A similar experiment. In a bottle marked E, a plant of Jungermania placed in 2 oz. of water on July 9th.

July 11th.—Monads.
„ *12th.*—More Monads, on small green Gonidium.
„ *15th.*—Monads, active.
„ *23rd.*—Monads.
Aug. 4th.—Small Oscillatoria.
„ *11th.*—Lyngbya threads and numerous gleocapsoid cells.

In observations made during the last two years I find that Euglena is developed by growth of cells, apparently from the transparent Monas cells given off by Lyngbya, Vaucheria, Jungermania, Conferva rivularis; in some cases, however (that of Conferva rivularis), it is produced from the vacuole, very similar to Dr. Bastian's illustration.

In proof of the universality of Monad forms as arising from the various organized substances, and thus supporting the "ab ovo" theory, I may refer to Dr. Bastian's experience (p. 223) of the appearance of "the minutest dot-like projections," which became vesicular, granular, and nucleated, arising from a Leptothrix thread.

Nov. 1st, 1869.—I saw a tubule of Vaucheria burst and discharge Monads, many of them only distinguishable by change of light.

Feb. 11th.—Examined Leptomitus clavatus from a dead salmon, and found in the water Monads as pin points visibly moving about one another.

The lungs of a rabbit in water produced Vibrions and Paramœcia.

Minute dots visible only by reflected light collected from the air (see M. M. J., Jan., 1870, Art. V., p. 25).

Dr. Hughes Bennett says,* "Although the constituents of air vary in different places, infusoria produced in all of them are identically the same."

Thus we have protomonads from Dr. Bastian's solutions, from Vaucheria; from decayed fish, from decomposing meat, and from air the same "minute dots," living specks, or invisible germs; and that these "minute dots," dot-like projections, &c., are the early stages of the developed forms of Monas Lens, Kolpoda, Uvella, Paramœcium, Amœba, Gonidium, Euglena, Oscillatoria, Lyngbya, and many of the Vorticellæ, I think no one who has been in the habit of watching vegetable infusions and the unicellular Algæ can doubt. And in these observations we see so much of the law "ab ovo" that it renders us still rather sceptical as to the validity of the "de novo" theory.

The following experiment may be of interest:—

July 26*th*.—Put a solution of tartarate of ammonia into a 2-oz. bottle, half air; corked and sealed it up.

July 30*th*.—Liquid clear. A few patches of a sort of pellicle. No Leptothrix threads, or any moving life.

Aug. 13*th*.—Liquid opalescent, as if milk had been added. No distinct Leptothrix. A large quantity of granular matter, as if a pellicle.

At the same time put 1 oz. of the solution into 2-oz. bottle, with six drops of Condy's fluid. I then passed a piece of oiled silk, containing some charcoal heated to redness, with some carbolic acid. Then corked it, and sealed over tight with sealing-wax.

On August 5th I passed two needles through the cork, and connected them with a slight galvanic current.

Aug. 14*th*.—The liquid is perfectly clear, but of a light straw colour, from the permanganate.

I also placed a portion of the liquid in an evaporating dish, and on August 11th found a large quantity of Leptothrix filaments.

One suggestion occurs to me in reading Dr. Bastian's remarks, September 22, 1870:—

The heat of 153° C. may so alter the primordial utricle of the cell as to devive it, but after a time in a cooler temperature it may be revived; the vital power not having been so far destroyed as to admit of the destructive agents from without overcoming the constructive power from within.

* 'Pop. Sci. Rev.,' Jan., 1869.

V.—*American Microscopes and their Merits.**

By Charles Stodder.

The first of the papers given in the footnote is an elaborate attempt at an account of American microscopes and their merits; but should have more properly been entitled an attempt to describe the microscopes made by R. B. Tolles, as of the twenty-five pages which it covers, twenty are given to Tolles. The second article, above named, but first in time, is noticed here merely to illustrate some points in the first, and the third, because it was preliminary to the first, which only elaborates more in detail what Dr. Hagen said in his verbal communication, and repeats statements and assertions which at the time they were made, Dr. Hagen was informed, by those as fully competent, to say the least, as himself, were erroneous; but in this first-named paper Dr. Hagen sees fit to entirely ignore the refutations, and makes the same statements deliberately again, as though there had been no contradiction of them. There is no other course left for those who know him to be wrong or feel aggrieved by his statements, than to examine his qualifications for pronouncing judgment, and to show wherein he is mistaken.

Dr. Hagen being a man of acknowledged scientific acquirements, and holding a reputable position at Cambridge, his opinions, given on a professed detail of facts, and after a *claimed careful study* of two years, published in a journal of high repute in Europe, will command attention and respect there, among those who have no opportunity to see and judge for themselves. If he had stated facts correctly, his paper might have been left to itself to refute his "opinions." No one can object to any comparison of American instruments with others; it is only asked that the comparison shall be made fairly, and by a competent expert. The writer proposes to show that Dr. Hagen's investigation has been superficial and inadequate to the task he undertook; and that he has mistaken facts and repeated assertions after he had been informed that they were erroneous.

Dr. Hagen opens his first communication to the Boston Society of Natural History by saying:—"Having worked with the microscope more than thirty years for medical and scientific purposes—following the gradual perfecting of the instrument—I was anxious to examine the power [?] of American microscopes."

"During the past ten years there has been great competition

* "On the North American Microscope;" by Dr. H. Hagen, Cambridge, Mass.: Max Schultze's 'Archiv für Microscopische Anatomie;' Bonn, 2nd No., 1870. A communication by Dr. H. Hagen on his experience in the use of the microscope: 'Proceedings of the Boston Society of Natural History,' vol. xii., p. 357. March 10, 1869. A verbal communication on Tolles' and Scheick's microscopes, to the Boston Society of Natural History, November 10, 1869 (unpublished).

among opticians, but in every case their progress has been arrested by one insurmountable obstacle." [What one?] "Since the *recent* improvement in correcting objectives for the thickness of covering glasses, comparatively little has been done." Why he should have restricted the "great competition" to the last ten years, and called the improvements in objectives "recent," when the competition in London has been active for forty years, and the "improvement" was made by Ross nearly or quite thirty years ago, can only be explained by supposing what has been generally believed to be the fact, that the "improvement" and the competition had not reached Germany until the last ten years. So far from little having been done since the "improvement," so much *has been done in England* that the Royal Microscopical Society of London, which procured objectives of the "three" leading London artists in about the year 1845, in 1867–8 abandoned the whole of them as behind the times, and obtained new ones of the same makers.

Dr. Hagen then makes some very just observations on "the difference in the aberration of the eyes of the observers. There is no doubt that different observers obtain different results with the same instrument." This is an important fact and an important admission from Dr. Hagen. It is well known to many microscopists, but is generally ignored. It is a pity that it did not occur to Dr. Hagen to remember what he had written in March, when he in October recorded some of his own observations.

The paper in the 'Archiv' begins by saying that for the past twenty years the "prominent excellence of American microscopes has been frequently mentioned" and it has been "asserted that their achievements have essentially excelled those of European make." "To my knowledge a direct proof of this has never been exhibited, it has not been shown that anything has been ever better seen than with European instruments." "Thus the American instrument constituted until recently a myth towards which all interested in this branch of science gazed with anxious curiosity, and prompted me during my two years' residence in this country to become *thoroughly acquainted* with it, and I have *spared no pains* to study them carefully." Here we have distinctly the task set forth, and the claim that he spared no pains to accomplish it. Let us see what were the "pains" taken. "The members of the Microscopical Section of the Boston Society of Natural History, especially Mr. Bicknell of Salem, Mr. Greenleaf of Boston, Professors Agassiz and Gibbs, Mr. Edwards of New York, and Mr. Tolles himself, have kindly seconded my efforts." Four of these gentlemen certainly were competent to assist. The writer cannot say what Mr. Edwards or Professors Agassiz and Gibbs did for assistance; but he states positively that neither Mr. Greenleaf nor Tolles "assisted;" that Mr. Bicknell was the only one of the three who had

any intimation whatever of Dr. Hagen's intention of becoming "thoroughly acquainted" with the American microscope, for the purpose of publication; they were never *asked to assist* for any such purpose. Had Dr. Hagen not spared his "pains;" had he inquired for those who could have "assisted" him in his "study" and have given him "positive proofs," he would have been referred to Professor Holmes and Professor Bacon of his own university, and to Professor Smith, of Hobart College, New York—*microscopists* who have made a study of the microscope for twenty years—to Dr. Barnard, President Columbia College, New York; to Professor H. J. Clarke, of the Kentucky University; to J. E. Gavit, Esq., of New York; to Dr. F. W. Lewis, of Philadelphia; to Professor C. Johnston, of Baltimore; to Mr. J. S. C. Greene, jun., of Boston,—gentlemen who have made the comparison of European microscopes of the *best makers*, with American instruments almost a specialty; had he done this his study might have produced more correct results; that is, if he had given heed to the information he received—for he seems to have disregarded that which he obtained from Messrs. Greenleaf and Bicknell.

Dr. Hagen gives his "general opinion" before giving the details, and says "novelty of any importance is not obtained." Yet before he concludes his paper he enumerates six novelties, all invented or designed by Tolles; namely, his binocular eye-piece, the illuminator of opaque objects with high powers, the low-power immersion lens, the solid eye-piece, the mode of effecting adjustment for covering glass, and the amplifier, and overlooks others quite important by Tolles and Zentmeyer.

"Objectives and oculars accomplish with slight variations as much as the best European, never more; on the contrary, English and French objectives have accomplished some things which the Americans have hitherto failed to do." It is not the purpose of this paper to produce evidence outside of Dr. Hagen's own statements, as to what American objectives have done. It is only needful to contrast what he says above with what he says he himself saw. Dr. Hagen says "that an objective $\frac{1}{10}$th inch with ocular C. showed while band 19 [of the Nobert test-plate] was in the centre of the field, the 18th, 17th, and half of the 16th bands; the lines in all were *well defined*, but not so that I could have counted them all. I could *count about forty* of the 19th, the rest blurred." "None of Tolles' objectives have well resolved the 16th to 19th bands of Nobert's plates, which has been done with the $\frac{1}{16}$th of Powell and Lealand." It would seem incredible that the same person could have written the above lines in the same paper; most especially after he had been positively informed by five gentlemen that they had seen the 19th band resolved, and with *several* of Tolles' objectives. But Dr. Hagen takes the ground (though not

in this paper, as he should have done) that because *he* did not *count all the lines* at once, they were not resolved; and it is true that he is not alone in that theory. To show the absurdity of this we will suppose that Nobert had ruled in the 19th band only 28 lines instead of 57, would Dr. Hagen say they were not resolved, when he saw the whole, because there were no more? Or if Nobert had covered a whole inch with the 112,000 and some odd lines, would anyone claim that they must all be seen at once? If either of these suggestions is answered in the negative, then Dr. Hagen has himself seen the 19th band resolved with a Tolles' objective. But Dr. Hagen says that American objectives have done "never more than European," and yet what he did with a $\frac{1}{10}$th objective is much "more" than to see all the lines with a $\frac{1}{16}$th (really a $\frac{1}{20}$th). He never saw, read of, heard of a $\frac{1}{10}$th European objective that would do what that one accomplished. This is not all; his sight of the *Surirella gemma* gives the same contradiction to his "opinion." He says, "*S. gemma* with the same $\frac{1}{10}$th showed only in a few places oblong fields between the cross-lines, but not well defined or regular as in Hartnack's drawings." Well, did anyone ever see them so? If Dr. Hagen knew as much of diatoms as of insects, he would have been aware of the fact that Hartnack's figure is a theoretical diagram, not a representation of the appearance in the microscope. Probably the only person living who claims to have seen what Hartnack calls the "flat hexagons," is Mr. Bicknell, who says he saw them, and only with a Tolles' $\frac{1}{12}$th. Hartnack does not say distinctly that he has seen them with a $\frac{1}{16}$th; he attempted to show them to two accomplished microscopists, and both failed to see them. Dr. Eulenstein has also failed with Hartnack's Nos. 10, 11, and 12, Powell and Lealand's $\frac{1}{50}$th and Ross' objectives; and Dr. Hagen knew these facts, for the writer told him before his paper was written; comment is unnecessary. Dr. Hagen also says that Hartnack's $\frac{1}{16}$th has resolved *S. gemma*, and Tolles' $\frac{1}{10}$th has not, *ergo* Hartnack's has done what Tolles' could not. Dr. Hagen has himself furnished the "direct proof" he wanted of the "unsurpassed excellence" of the American objective.

Now for some of Dr. Hagen's errors and mistakes. He says of Tolles' objectives "the workmanship is superb," "the adjustment only moves the lower lens from the two others." The solid eye-pieces are "really bi-convex Coddington lenses." He gives on the authority of Edwards a formula of Tolles' objectives; all there is to be said is, that the formula is not Tolles' formula, the eye-pieces are not Coddington lenses, and that Tolles had never made objectives to move the front lens; all of which Dr. Hagen could have easily ascertained.

Dr. Hagen considers that "a most important fault of the instrument consists in the difficulty of its use. In order to adjust them

so that they will give their greatest results requires delicate labour and considerable time. In this respect they are excelled by the higher as well as the lower powers of English and German." "The ease of treatment of Hartnack's and Schieck's highest objectives is certainly far less troublesome." If this means anything it must refer to the delicacy of the adjustment for covering glass. Undoubtedly Schieck's are far less troublesome. It is thought to be well known to microscopists that the delicacy of this adjustment—consequently in one sense the difficulty of use—is increased just in proportion to the approach to perfection of the lenses. Certain it is that Hartnack when delivering an objective made for a member of the Boston Society of Natural History two years ago, called the purchaser's attention especially to the fact that when an object was best shown, the movement of the adjusting ring one hundredth of an inch either way destroyed the effect, as an evidence of the perfection of his work. As to English objectives. Dr. Pigott, in a recently-published article on high-power objectives, speaks of a certain effect being entirely destroyed by a change of this adjustment which moved the lens only $\frac{1}{1400}$th of an inch. So much for English lens and Hartnack's. Microscopists know that Dr. Hagen is in an error as to good objectives, but correct if his remarks are applied to poor ones; and it is not surprising that he was "*utterly astonished* to see how much more the hand of the artist himself will develop with the instrument."

The majority of the microscopists here are "dilettanti or workers on diatoms;" this must be news to Professors Holmes, Bacon, Ellis, and Gray, and to their hundreds of past and present students; the "truth will be respected" if it is said that there are hardly enough diatomists in the whole country to encourage each other.

Dr. Hagen thinks that his attempt at "even pronouncing a judgment on the local instruments caused a storm of indignation against me by the resident microscopists," and accounts for it by the assertion that "*we know* that most of them are members of the Boston Optical Association." Dr. Hagen here refers to the reception of his verbal communication to the Boston Society of Natural History in November last. Of all the persons then present but two were members of that Association, and whatever indignation was manifested was at his preposterous comparisons of cost. Dr. Hagen then asserted that the American instruments cost 600 per cent. more than German of equal merit, and that "English objectives of the most celebrated makers could be imported to advantage." In his paper in the 'Archiv' Dr. Hagen reduces the comparative cost of German and French objectives to "one-third or one-fourth as much," but repeats his comparison as to the English "according to Frey's statement." Now before this paper was written the cost of importing English objectives was read in detail to Dr. Hagen, and it was

shown from the makers' price lists that the cost was much higher than Tolles' prices for similar objectives, and yet Dr. Hagen elects to repeat his erroneous statement. He said then that he "spoke for the interest of science." Can the interest of science be promoted by such misstatements? It was not the intention of the writer to have said anything more on the matter of cost, but while writing this paper a letter was received, an extract from which is a good comment on all that Dr. Hagen has said as to cost and workmanship. It is not known that the writer of the letter ever heard of Dr. Hagen or his comparisons. The letter was written by Colonel J. G. F. Holston, M.D., Washington, D.C., June, 1870. "I was never dissatisfied either with Tolles' prices or his workmanship, for although apparently dearer than some other makers, the superior excellency more than balances it. I can do with my $\frac{1}{12}$th by Tolles (cost $100) all that Powell and Lealand's $\frac{1}{50}$th will do well that cost the United States $350. I compared them myself at the museum." Dr. Barnard, President of Columbia College, New York, writes:—"Dr. Hagen is absurdly wrong in his comparison of the performance of the American and foreign objectives of the same price." "It is nonsense to make such comparisons as these price for price."

No less unfortunate is Dr. Hagen in his description of Tolles' first-class instruments; he partially describes the plan and construction of some instruments which he had seen—omitting, however, some of the most peculiar details — and mixing with that some of the peculiarities of a unique instrument, the only one of the kind ever made, and which he has never seen, the particulars of which he could have got from Dr. Barnard's report of the Paris Exposition of 1867—constructing in this way an instrument which has no existence. He claims to have "seen and tested nine of Tolles' instruments of the largest class." The writer will not say that is impossible, but he can say that there are no nine instruments of the "largest class" known to Mr. Tolles that Dr. Hagen could possibly have seen and tested. His classification must be treated as an error until he furnishes a list of the nine. The self-sufficiency with which he charges the reverend President of Columbia College with making, in his official report of the Paris Exposition, a claim that is "hardly tenable" is, to use his own expression, "quite comical." Dr. Barnard had reported that "it was to be regretted that the American makers did not send" stands to the exhibition; for the want of them the objectives were not properly examined. Dr. Hagen twists this round in this way, "The same objectives are frequently used here with English stands and oculars, plenty of which were to be had in Paris. If, then, they did not prove themselves successful, the *reason must be* that they did not attain as much as others. The circumstances of the difficulty of

their adjustment is not to be allowed in this case, as the reporter (Barnard) himself is an adept in their use," all of which is entirely imaginary with Dr. Hagen. A recent letter from Dr. Barnard recites the whole story. He says:—" In regard to what Dr. Hagen says of my report, he so singularly misunderstands me, or so wilfully misrepresents, that it seems hardly necessary to reply to him. I never said or intimated that a Tolles' stand was necessary to develop a Tolles' objective, but only that a stand of some kind was necessary, a proposition which I think stands to reason. The disadvantage could not appear until the jury, instead of examining the glasses, country by country, as I supposed they would, using certain uniform tests, ordered at once all the exhibitors of microscopic objectives to present their glasses simultaneously in one place (and that, by the way, as bad a place as could be selected—a small room with one window, a moderately-sized table, and no chairs). Had the first plan been pursued there would have been no trouble about stands, for Mr. Beck, of London, was close by the American section with a case full of apparatus, including stands of all forms, one of which he subsequently placed at my disposal for some length of time. But when the crowd came together at the place appointed, the American glasses were present without any stands; and though both Mr. Ross and Mr. Beck, *after their own glasses had been examined*, permitted me to make use of their stands, the weariness of the protracted examination, with the extreme heat of the crowded room, made the jury impatient, and notwithstanding the compliment Dr. Hagen pays me as an 'adept,' I was not smart enough to secure, on that occasion, what I thought a fair trial of the glasses—by which expression I mean *not* a fair development of their powers, but a fair *attention* to their development. *I never got the whole jury to examine the glasses thoroughly.* After I had obtained from Mr. Beck a stand, Dr. Brooke, of London, made the fullest trial with them which I could secure from any member, and he expressed himself favourably, though he has the natural national leaning of an Englishman. It would have been ridiculous for me to narrate all this in my report, but it is absurd for anyone to interpret what I do say as Dr. Hagen does." That effectually disposes of Dr. Hagen's inferences that the American objectives "did not attain so much as others."

Dr. Hagen attempts to controvert the opinion now unanimously received in England and America, that the microscope should be so constructed as to receive an inclination. He says, "The statement made by people here that the working with high-stand instruments (they being turned back) is much more convenient, as keeping the neck straight prevents the rush of blood to the head, makes rather a comical impression. I say comical, when we consider that for tens of years back several thousand low-stand instruments have been

in daily use in Europe without detrimental results." [?] Possibly no one but Dr. Hagen has ever heard that the use of vertical instruments caused a rush of blood to the head; but the experience of all microscopists here (Dr. Hagen excepted) is against the use of the low-stand vertical instruments, and that evils and imperfect work do result from the use of such. To show that the "comicality" of the objection is not original with American microscopists, the following is extracted from Dr. Wm. B. Carpenter's work on the Microscope,—an author whose opinion is certainly equal to Dr. Hagen's thirty years' experience—written fifteen years ago. "Scarcely less important . . . . is the capability of being placed in either a vertical or a horizontal position, or at any angle with the horizon, without deranging the adjustments of its parts to each other." . . . . "It is certainly a matter of surprise that opticians, *especially on the Continent*, should have so long neglected the very simple means which are at present commonly employed in this country of giving an inclined position to microscopes, since it is now universally acknowledged that the vertical position is, of all that can be adopted, *the very worst*." Perhaps if Carpenter's work had been translated into German fifteen years ago it might not have been needful to write this paper.

Dr. Hagen has so little to say of American microscope makers, other than Tolles, that he found it impracticable to make so many mistakes in regard to them. If he had taken more "pains" he could have added materially to the number.

Of Spencer he says:—"A few years ago, however, he retired from the business." This is a mistake, for which probably Dr. Hagen is not responsible. "I have not, in fact, had an opportunity to compare Spencer's objectives and oculars." "In Boston, Salem, and Massachusetts generally, there are none of Spencer's instruments to be found;" that is because he "spared the pains" to find them. The writer had them, and would have guided the inquirer to others.

Of Zentmeyer he remarks:—"As near as I can find out he makes no glasses. Each of his stands that I saw had objectives and oculars of Tolles or Wales." Another example of the superficial knowledge obtained by Dr. Hagen; a portion of the very oculars which he saw on Mr. Bicknell's instrument, and which he gives the power of as Tolles, were made by Zentmeyer! Had he not "spared pains" to inquire, he could have learned that Zentmeyer does make glasses, and that one of the Tolles' stands which he had seen was furnished with an excellent objective by Zentmeyer. In the notice of Zentmeyer's stand the most important and characteristic features are entirely unnoticed!

In his notice of Grunow's instruments he particularizes an inverted microscope, the peculiarity of which was a movement by

friction rollers, an invention of Tolles, and which he (Hagen) had seen various modifications of on several of Tolles' instruments, in particular the first one in which it was ever introduced; yet he failed to notice it there.

It may, perhaps, be urged for Dr. Hagen that these things are trivial, and to some they may look so; but they constitute Dr. Hagen's paper; the aggregate of the trivialities makes about the whole. Dr. Hagen fails throughout all his papers to appreciate the difference between magnifying power and quality.

With a patronizing air that is "nearly comical," after reading the paper, he compliments the artists in these words:—"Messrs. Tolles and Wales are no doubt artists of the first water, constantly endeavouring to advance and enlarge their science."

Dr. Hagen admits that he has not exhausted his subject, and promises to renew it; it is to be hoped that he will, and that when he does he will spare no pains to make himself thoroughly acquainted with it; if he endeavours to do that, all our microscopists will cheerfully assist him.—*American Naturalist*, vol. iv., No. 7.

NEW BOOKS, WITH SHORT NOTICES.

A Manual of Human and Comparative Histology. Edited by S. Stricker, Vol. I., translated by Henry Power, M.B.Lond., F.R.C.S., Examiner in Physiology in the University of London. The New Sydenham Society, 1870.—Mr. Henry Power, the accomplished translator of this the first volume of Herr Stricker's work, has done excellent service to English Histology by rendering the present work into English. It is quite true, as he says, that we have such excellent works as Kölliker, translated by Messrs. Busk and Huxley, in our language. But the translation of this, though excellent so far as Messrs. Busk and Huxley's labours are concerned, is certainly a little behind the time in some respects. It was quite right that the excellent volume of Stricker's within a year of its completion in Germany should appear, and it is so very perfect a work of its kind, and will we doubt not, when completed, be so admirable a treatise on Human Histology of the most advanced schools, that it merits the attention of all who have even the smallest interest in microscopic anatomy. It would be impossible for us to attempt a review of the whole treatise. Indeed, so far as we can see, there is not very much novelty in many of the chapters. But in some it is unquestionable; for indeed, so far as our text-books are concerned, there is none which at all completely contains the facts put forward in this volume. Foremost among the chapters which strike us as particularly novel, comes the communication of Max Schultze on the general characters of the structures comprising the nervous system. This is particularly interesting, if only from the fact that in most of our treatises it is dealt with in so particularly ancient a fashion, and Herr Schultze has so much that is new to most English readers to say about it. In regard to Pflüger's views, first published in 1866, Max Schultze makes the following remarks:—"It is this kind of nerve-fibre which with few exceptions is present amongst the Invertebrata. Nerve-cords which consist of such fibres do not possess the bright glaring appearance of ordinary nerves, but are semitransparent, grey, gelatinous, and resemble embryonic tendinous tissue. If they are freed from the denser connective tissue which invests them, they can be broken up into their constituent fibres as easily as other nerves, which is a consequence of the firm consistence of the sheath of Schwann surrounding each fibre. The diameter of these non-medullated nerve-fibres, varies very considerably. In the Sympathetic they scarcely exceed that of the medium-sized medullated fibres, but in the olfactory nerves of many animals fibres may be found at least three or four times thicker than the largest medullated fibres." The author then points out where these are best found, and figures them, and he concludes this portion of his essay by a classification of the several fibres, which he arranges under six different classes. In regard to the minute structure of the ultimate nerve, it is

interesting to observe that even the author and Stilling, though holding in common the composite character of the axis cylinder, differ from each other as to its arrangement, for while Schultze regards this as being composed of fibres simply, Stilling looks on it as partly consisting of fibres, but as arranged in a peculiar fashion.

In regard to the division of nerves, the most remarkable instances occur among the lower animals, and some of these are here recorded by the author. The nerve-hairs of young fish and naked amphibia are also referred to by him, but we should think much too briefly, for this is a subject of very considerable interest to the comparative anatomist. As regards the termination of the nerves much information is given. Especially valuable will, we think, be found the admirable illustrations of the arrangement of the nerves in the electric organs of certain fish of the genus *Mormyrus*. The same may be said of the section upon the mode of origin of the nerve-fibres in the nerve-centres. This is a subject upon which, much as has been written, very little is known for certain. It is illustrated by some very remarkable and interesting woodcuts, some of which, as those of multipolar ganglia (taken from *Deiters*), are most remarkably suggestive. All through, this chapter contains much that is interesting and novel, and it will be read with advantage by those concerned in nervous research.

We had hoped to have been able to give some of the details upon the Lymphatic system, but we fear that we have already exceeded our normal space. All we say in conclusion is, that the treatise is a most valuable one; that Mr. Power has performed a tedious and extremely difficult task not only with excessive ability but with great taste, and we hope that his ideas with regard to the appearance of the next volume may fail in nothing. The book should be in the hands of every student of Histology.

PROGRESS OF MICROSCOPICAL SCIENCE.

On the Taxonomic Value of the Sheath of the Œsophagus, especially as regards Sauropsida.—This paper, in the last number of the 'Proceedings of the Zoological Society,' is an addition to the many proofs that Professor Gulliver has given of the use of the microscope in systematic zoology and botany. Referring to the results of his researches, published in the same 'Proceedings,' June 14, 1842, and April 22, 1869, he shows that a sheath of transversely-striped muscular fibre exists to a greater or less extent on the œsophagus of Mammalia and Fishes, while there is no sheath of such fibre on the œsophagus of either Birds or Reptiles. In Mammalia the sheath of striped muscular fibre extends either down to and even on the cardiac end of the stomach, or stops short at a more or less distance from the cardia;

and so regular and constant is the arrangement in this respect as to afford good diagnoses between different orders or families of this class. Thus, *e. g.*, in Ruminantia and Rodentia the sheath of striped muscle clothes the entire length of the œsophagus; while in Man, Quadrumana, and Equidæ, this sheath stops much short of the stomach. Of the Felidæ the œsophageal sheath of striped muscle does not, though in the Ursidæ it does, extend to the stomach. The author thinks the whole subject the more deserving of inquiry, because it is a neglected point in systematic zoology, and is generally treated in a perfunctory or erroneous manner. For example, in the current 'Anatomy of Vertebrates' we have only a notice of the sheath of striped œsophageal muscle of Ruminants, as if several other mammalian orders were destitute of it; and the error is extended by a false comparison of this sheath of certain Birds with that of Ruminant Mammalia. But, in fact, the want of this sheath is a characteristic of Sauropsida; and it is remarkable that while this section of Vertebrates is thus deficient in striped muscular fibre on the œsophagus, the presence of this kind of muscle within the eye of Sauropsida is another distinction between them and the highest and lowest classes of Vertebrates.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *October* 12, 1870.

James Glaisher, Esq., F.R.S., Vice-President, in the chair.

In opening the meeting, Mr. Glaisher stated that he had been requested to preside on that occasion in consequence of the inability of the President, from ill health, to do so. Mr. Reade, however, wished that his respects should be presented to the Fellows, and that his feeling of regret at not being able to meet them should be expressed. He (Mr. Glaisher) was sure that the good wishes of the President would be reciprocated, and with the hope that at the next meeting he might be in a condition to resume his official position.

It was announced by the Chairman that the volumes of the 'Philosophical Transactions' which had been presented to the Society by the President, were now placed upon the shelves of the library, and were open to the inspection of the Fellows.

The minutes of the last meeting were then read and confirmed.

A list of donations made to the Society was read, and the thanks of the meeting presented to the respective donors, special mention being made of an interesting old volume entitled 'Abhandlungen von Insecten,' by Jacob Schäffer, given by Dr. Millar; and also of some

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

deep-sea dredgings made by Lieut. Baker, R.N., off the coast of Malabar.

The Chairman announced that a communication had been received from Mr. Hankey, of Chicago, recently elected an Hon. Fellow of the Society, offering as a token of his interest in the Society a prize of five guineas for the best essay "On the Microscope in Education." The thanks of the meeting were unanimously given to Mr. Hankey for his offer, which, the Chairman stated, would be considered by the Council, and the conditions under which the essay was to be written made known as soon as their decision was made.

It was stated that a paper by Mr. J. Beck, "On the Mode of Obtaining Definition of the Structure of the Scales of Thysanuræ," would, with his consent, be taken as read, printed in the Journal, and discussed at the next meeting, in conjunction with one on a similar subject by Mr. McIntire.

Mr. W. S. Kent then read, in brief abstract, a paper "On the Coralliospongiæ, or Anastomosing Sponges."

The Chairman, on thanking Mr. Kent on behalf of the meeting, expressed a hope that the results of his deep-sea dredging expedition would be published *in extenso* in the 'Transactions' of the Society, and be made the subject of a discussion at some future meeting of the Society.

Dr. G. W. Royston-Pigott then gave a *viva voce* exposition of a paper "On Aplanatic Illumination and Aplanatic Definition."

Mr. Browning inquired whether, in regard to the size of spurious disks, Dr. Pigott did not think it was dependent upon the diameter of the object-glass, and that consequently the larger the angular aperture of the object-glass, the smaller the disk would be. He wished also to express his admiration of the ingenuity of the experiments which Dr. Pigott had made on the subject of his paper.

Dr. Pigott stated in reply that he had in telescopes observed the effects mentioned by Mr. Browning, and that he had found the size of the disk diminish as the aperture of the object-glass increased. But the experiments upon spurious disks were attended with some difficulty, for the laws as to these disks in the microscope differed from those which obtain in the telescope, since the microscope defines under totally different conditions. He believed he was correct in stating that no telescope ever got an angle of aperture of more than 7°, some indeed had only 5°; whereas in the microscope you might have an object-glass with 170° of aperture.

Dr. Pigott also alluded to definition under the binocular microscope, and stated that Dr. Carpenter had told him that he was unable to define the spherical form of Polycystinæ until he requested Messrs. Powell and Lealand to make for him a $\frac{1}{2}$-inch object-glass of only 40° aperture; larger apertures failed.

The Chairman said he should like to have some explanation of the construction of the aplanatic searcher.

Dr. Pigott replied that the aplanatic searcher consisted of a pair of achromatic lenses somewhat over-corrected. There was some subtilty about its application, and just in this way: If the lenses were

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drawn down towards the object-glass, sometimes he had found it requisite to separate the objective lenses a little more than usual. When the lenses of the searcher were drawn upwards, a finer effect was produced, on the contrary, by closing the objective lenses. Some little skill was required in getting the best results, because it was necessary to use the screw collar very carefully. On looking at the podura beading with the glasses in a particular position, an effect would be produced resembling the figure given by Mr. Wenham. These club-headed markings were obtained easily enough by altering the corrections with the aplanatic searcher. But with different corrections the beaded structure may appear.

The Chairman inquired whether the screw separated the two lenses of the searcher?

Dr. Pigott: Yes; but care should be taken in separating these two lenses to keep the corrections conjugate, and correlative with those of the objective.

Mr. Brooke asked what the nature of the aplanatic searcher was. Was it a combination of two lenses?

Dr. Pigott: Both the lenses are formed of crossed lenses, cemented to flint-glass concaves.

Mr. Brooke: What is the focal length of these lenses?

Dr. Pigott: Combined, about an inch and a quarter; the distance between them about half an inch.

Mr. Brooke: Are they of equal power?

Dr. Pigott: No, the back one is the weaker; and they are combined in different ways to make them more or less corrected. One thing is to be considered, *viz.* keeping up the flatness of the field. This effect will vary much, according to the eye-piece that is in use.

Professor Huxley, addressing the meeting as a visitor, said, that being practically interested in researches such as those which Dr. Pigott was conducting with so much ability, and having seen his paper in the 'Proceedings' of the Royal Society, he had been much struck by it, and being very much in want of means of looking through tolerably thick glass under a high magnifying power, he made it his business to ask Professor Stokes about Dr. Pigott. The reply was so favourable that he wrote to Dr. Pigott for assistance, by whom he was told to apply to Messrs. Powell and Lealand, who made an aplanatic searcher for him; and he (Professor Huxley) having used it frequently, must bear witness that there was no sort of doubt as to the wonderful illuminating power it possessed, combined also with the great magnifying power which could be got out of a comparatively low object-glass by the use of this instrument. But when the attempt was made to go farther, that is to say, when the instrument was applied to deeper object-glasses (it might be from want of proper knowledge in the use of it), it did not seem to be of much use.* It was an exceedingly important practical question at the present time,

* Since the meeting at which these words were spoken, I have had the great advantage of applying the "searcher" to deep objectives under Dr. Royston-Pigott's guidance, and I am disposed to form a very much more favourable opinion of its utility.—T. H. H.

and he was inclined to think that in histology, for the purpose of analyzing organic structures, the existing microscopes were, as the Yankees would say, "played out." We have got as far as they will take us. He believed that a $\frac{1}{50}$th did not enable anybody to see anything which could not be seen with a good $\frac{1}{12}$th of Ross. He considered these deep objectives to be eminently delusive, and they were so doubtless for the reasons which had been stated by Dr. Pigott. He could not doubt that Dr. Pigott had got on the right track of showing what was to be done in the present state of affairs. Practically the nature of the question just now is whether, in an organic tissue, one could truly define a point not more than $\frac{1}{50000}$th of an inch in diameter. There was always that unhappy luminosity about the margins of such objects, which he did not doubt arose from the causes which Dr. Pigott had pointed out. Histologists, he feared, were at the end of their work unless, by the aid of some such appliance as Dr. Pigott had endeavoured to furnish, they could obtain microscopes which would enable them to separate two points the 100,000th of an inch apart. Only then could they say whether the object was homogeneous or not. At present when they talked about homogeneous solids or fluids, or attempted to define an object like *Bacterium*, they were absolutely in cloudland. He had come to the meeting in the hope that he might hear that some light had been thrown upon this subject; and he did indeed trust that Dr. Pigott had proceeded some way on the road towards the solution of the difficulty; at any rate, he had *macadamized* the road, and that was a great matter.

Dr. Pigott, thanking Professor Huxley for the manner in which he had spoken of him, said he could well understand that so ardent a worker should feel the urgency of those wants to which he had referred. He thought, however, he might be allowed to say that in correcting the aberration of objectives, if there was nothing else to point to than a power of varying chromatic effects by means of the aplanatic searcher, a great improvement would have been made. The object-glasses of the present year were greatly in advance of those of previous years as regards the correction of visible error which must therefore have existed, however unsuspected and denied, for the very fact of their present superiority is a conclusive answer to the question as to whether any improvement had been made.

Mr. Slack asked Dr. Pigott to throw some light upon the subject of the colours which he described as preceding and accompanying the resolution of beaded objects.*

Dr. Pigott said: If a diatom slide as the Formosum were held obliquely, so as to let the sun shine upon it, then as the inclination of the slide was varied, the hues went through all the colours of the rainbow in regular and then in reversed order, thus exactly imitating the phenomena of the spherical falling drops of the rainbow, and proving the spherical nature of the beadings. He wished to make an announcement of the extraordinary power of varying the secondary spectrum shown by oil of cassia used instead of water, with the immer-

* Dr. Pigott had exhibited a diagram in which the singular isolated disks of the Formosum had been seen in sunlight.

sion lens; the cause of which lay in the peculiar refrangibility of coloured rays passing through the oil, as compared with water.

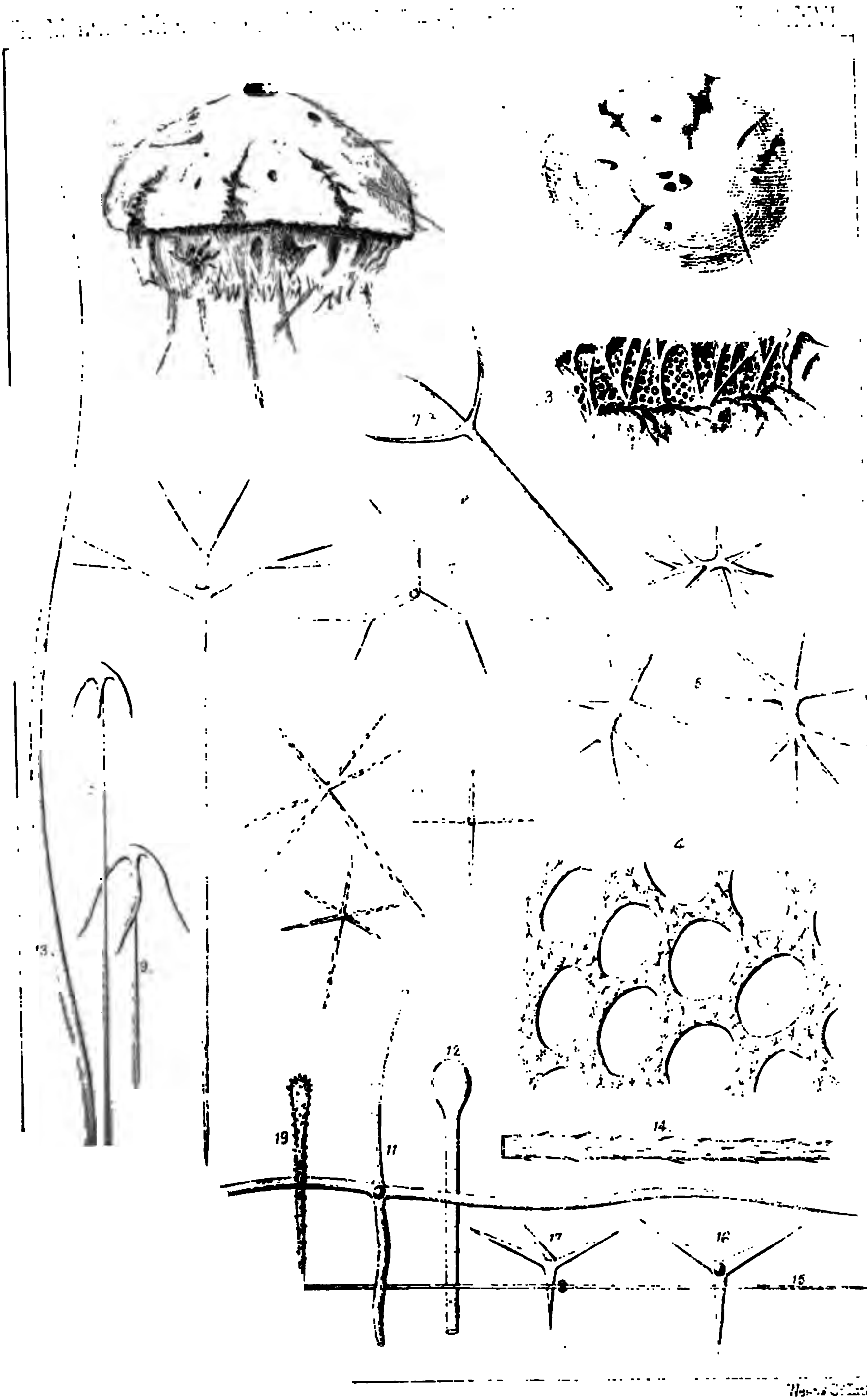
A vote of thanks was then passed to Dr. Pigott, and the meeting adjourned to November 9th.

Donations to the Library and Cabinet from June 8th to Oct. 12th, 1870:—

	From
Land and Water. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Transactions of the Linnean Society. 3 Parts	*Society.*
Quarterly Journal of the Geological Society, No. 103 ..	*Society.*
Microscopical Manipulation. By W. T. Suffolk	*Author.*
Le Globe.	
Journal of the Linnean Society, Nos. 54–5	*Society.*
The Practitioner, No. 27.	
Transactions of the Northumberland and Durham Natural History Society, Vol. III., Part 2	*Society.*
Bulletins de L'Academie Royal de Belgique. 2 Parts, 1869	*Academy.*
Annuaire de L'Academie Royal de Belgique	*Academy.*
Smithsonian Contributions to Knowledge, Vols. XIV. and XV...	*Institution.*
Smithsonian Reports for 1868	*Institution.*
Annual Report of the U. S. Department of Agriculture for 1868	*Institution.*
United States Patent Office Reports for 1867. 4 Vols...	*U. S. Patent Office.*
The Transatlantic Longitude. A Report to the Superintendent of the U. S. Coast Survey. By B. A. Gould	*Smithsonian Institution.*
The Gray Substances of the Medulla Oblongata and Trapezium. By Dr. J. Dean	*Smithsonian Institution.*
The Orbit Phenomena of a Meteoric Fire-ball. By J. H. Coffin, LL.D.	*Smithsonian Institution.*
Vargasia. Boletin de la Sociedad de ciencias Fisicas y Naturales de Caracas, No. 7.	
Commelinaceæ Indicæ, Imprimis Archipelagi Indici. By Carolo Hasskarl	*Author.*
Die Zoophyten und Echinodermen des Ardriatischen Meeres. By Prof. C. Heller	*Author.*
Vegetationsverhältnisse Von Croatien. Von Dr. A. Neilreich	*Author.*
Verhandlungen der kaiserlich-königlichen Zoologisch-botanischen Gesellschaft in Wien. 3 Parts for 1868–9.	
Proceedings of the Academy of Natural Sciences of Philadelphia. 4 Parts, 1869	*Academy.*
On the Structure and Affinities of the Genus Dicranograptus. By John Hopkinson	*Author.*
The Student, Nos. 3 and 4. New Series	*Publisher.*
Popular Science Review, Nos. 36 and 37	*Publisher.*
Abhandlungen von Insecten. By Jacob Christian Schäffer. 1764	*Dr. Millar.*
10 Slides of Fresh-water Algæ, &c., prepared and mounted by Mr. W. H. Walmsley, of Philadelphia..	*W. H. Walmsley.*
44 Slides of Marine Algæ, mounted by Mr. Chas. Adcock, M.R.C.S.	*Chas. Adcock.*

WALTER W. REEVES,
Assist.-Secretary.

[Owing to great pressure of matter, we are obliged to go to press without either our "*Notes*" or Reports of several Societies.—ED. M. M. J.]



'ew Sponges

THE

MONTHLY MICROSCOPICAL JOURNAL.

DECEMBER 1, 1870.

I.—*On a New Anchoring Sponge, "Dorvillia agariciformis."* By W. Saville Kent, F.Z.S., F.R.M.S., of the Geological Department, British Museum.

(*Read before the* Royal Microscopical Society, *Nov.* 9, 1870.)

Plate LXVI.

Dr. J. E. Gray placed in my hands yesterday morning, with kind permission to describe it, the sponge which, with an accompanying plate in illustration of its structure, I exhibit on the present occasion.

The form is a remarkable one. In external aspect it presents much the appearance of a "button mushroom" just springing from the ground, having several bundles of long filiform spicula depending from its lower surface. It may be said to be divided into two distinct portions: an upper, which takes the form of a hood, and corresponds to the "pileus" in making use of the fungoid comparison; and an inferior, bearing a rough resemblance to the stalk.

The supporting skeleton of this sponge is entirely silicious, having its component elements disposed in the following order. From the interior and basal portions we find radiating in every direction dense fascicles of stout spicula having their external extremity trifid and dichotomously branching, "Bifurcated expando-ternate" Bowerbank, as shown at Plate LXVI., Figs. 6 and 7. The interlacing ramuli of these spicules alone are exposed to the surface in the basal portion of the sponge. The surface of the upper part, however, presents quite a felty appearance, which arises from a mesh-work of anchorate, hexradiate, attenuate, and other varieties of spicula which are superimposed on the outer branching extremities of the trifid forms. The attenuate and anchorate spicula have frequently a definite fascicular arrangement. Immediately beneath the surface of the hood, and connecting it with the lower portion of the sponge, is a beautiful open reticulation of the sarcode, as shown at Fig. 3. Highly magnified, this net-work presents the appearance given at Fig. 4, and is found to be almost completely filled with minute irregularly stellate spicula, isolated and still more highly nified examples of which are shown at Fig. 5; this reticulation

of the sarcode most likely represents the porous system of the sponge, or in all probability constitutes the more important portion of it. In the firmer bands of sarcode intersecting the reticulation, as represented at Fig. 3, may also be found larger triradiate and quadriradiate spicula. The minute irregular stellate spicula are abundant in the sarcode throughout the sponge, in which also occur the small hexradiate and delicately echinate forms depicted at Fig. 10.

The rootlike bundles dependent from the base are found to consist for the most part of attenuate anchorate and attenuate linear spicula, having the minute hexradiate and irregularly stellate types interspersed among them.

The oscular system is very distinctly marked on the upper surface of the sponge, and, as may be observed in Figs. 1 and 2, consists of several large and unevenly distributed oval orifices, which give off branches and ramify internally throughout its substance.

In the specimen figured, the largest osculum occupies the centre, having the smaller ones distributed round it; in another example which accompanied it, however, there are four or five large oscula of equal size, and numerous smaller ones all irregularly disposed.

The possession of hexradiate spicula makes it necessary to refer this form to the group of the *Hexactinellidæ*, treated of at length in the November number of the Journal; but the oscular system and other structural characters are so entirely different from what obtains in all the species referable to that group which have hitherto been recorded, that it will be necessary to create a new division or family for its reception. This, however, with fuller particulars of its entire structure, I reserve for a future communication.

The sarcode throughout this sponge is remarkably firm in consistence, and quite distinct from that which characterizes those species of the *Hexactinellidæ* with which we are at present familiar. The diverse forms of spicula seem to indicate that this species constitutes a connecting link between the *Hexactinellidæ* and *Tethyidæ;* further study of it is necessary, however, before pronouncing a decisive opinion on this point.

I propose to distinguish this sponge by the name of *Dorvillia agariciformis:* the generic title, at the suggestion of Dr. Gray, in honour of Mrs. Dorvill, of Kingsbridge, Devonshire, a lady well known for her contributions to natural science, and more particularly for the beautiful figures she drew and etched to illustrate the 'Testacea Britannica,' and for the assistance with her pencil she rendered Col. Montague in his descriptions of sponges and other marine animals communicated to the Linnæan and Wernerian Societies. The specific one is given in consideration of its agaric-like form.

The specimens from which this description is derived have just been purchased by Dr. Gray, for the British Museum, with many

other British marine invertebrata; the locality from whence they were obtained is not yet known, except that they were taken at a depth of 540 fathoms.

EXPLANATION OF PLATE LXVI.

FIG. 1.—*Dorvillia agariciformis*, viewed laterally, enlarged ½ diameter.
" 2.—The same, as seen from above, nat. size.
" 3.—Under surface of the upper portion or hood, showing where it becomes continuous with the basal part, the netted structure of the sarcode × 3 linear.
" 4.—A fragment of this netted portion, highly magnified, and showing the numerous minute irregularly stellate spicula interspersed through it.
" 5.—Three of the minute irregularly stellate spicula × 500 linear.
" 6.—One of the large bi-ternately terminating spicula, of which the greater portion of the skeleton is constructed, × 20 linear.
" 7.—The same, viewed from above. 7*a*.—Portion of a simple porrecto-ternate spiculum from the same region, equally magnified.
" 8.—One of the smooth anchorate spicula, abundant in the dependent rootlets and on the superior surface of the sponge, × 40 linear.
" 9.—The anchorate termination of a variety of the same.
" 10.—Minute, rigid, delicately echinate hexradiate spicula, common in the sarcode of the sponge, × 100 linear.
" 11.—An attenuate, flexuose, hexradiate spiculum, from the upper and superficial surface, × 50 linear.
" 12.—The upper portion of an attenuate spinulate spiculum, from the same region, × 50 linear.
" 13.—A simple attenuate, common on the superior and superficial layer, × 50 linear.
" 14.—Portion of an attenuate adpressly spined spiculum, similar to what obtains in the genus *Pheronema*.*
" 15.—An attenuate linear spiculum, showing its tendency to develop into an hexradiate form, × 50 linear.
" 16, 17.—Triradiate spicula, from the reticulated layer of sarcode shown at Fig. 3, × 100 linear.
" 18.—A quadriradiate spiculum, from the same region, × 100 linear.
" 19.—A minute clavate and profusely echinate spiculum, from the sarcode, × 100 linear. (This form is too scarce to be considered absolutely characteristic.)

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* This spiculum is probably an interloper, as may also eventually prove to be the case with the form represented at Fig. 11. The great depth at which this sponge was taken, and the nature of the mud with which it is associated, makes it reasonable to anticipate that *Pheronema* flourished in its vicinity. The spined anchorate form of spiculum common to the last-named genus has also been detected.

II.—*On Aplanatic Definition and Illumination with Optical Illustrations.* By G. Royston-Pigott, M.D., M.A., &c.

(*Read before the* Royal Microscopical Society, *October* 12, 1870.)

Plate LXVII.

Mr. President and Gentlemen,

I have in the first place to express my regret that circumstances over which I had no control prevented me from accepting the invitation to be present at the discussion of the first paper I had the honour of sending for the consideration of the Council.

I have also to tender the Council my thanks for the present opportunity of explaining some views and methods applicable to Aplanatic Definition and Illumination. Important as is this subject, I fear that I shall fail to do justice to its merits to attention, though without doubt it forms the very pith and marrow of accurate instrumental vision.

At the outset I must ask your kind indulgence for the course I am about to pursue. There are many in this room who may perhaps think my conclusions turn upon points so elementary as to be unworthy of iteration; whilst many others may think the details, on the contrary, too technical or complex.

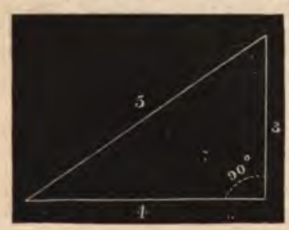
But, yet, I hope no apology is needed for appealing to fundamental truths. It is only by mastering the axioms of science that true progress can be made.

The man who starts on a voyage of discovery must provide himself not with instruments only, but with fundamental principles.

There are two fundamental cogent truths which have done more for exact science than any other *par excellence.*

Thus Astronomy, Navigation, Trigonometry, and Optics are built up upon that great property of right-angled triangles—that of the squares of their sides.

And Optics is especially dependent upon their relations to each other, or the rule-of-three proportion of their sides.

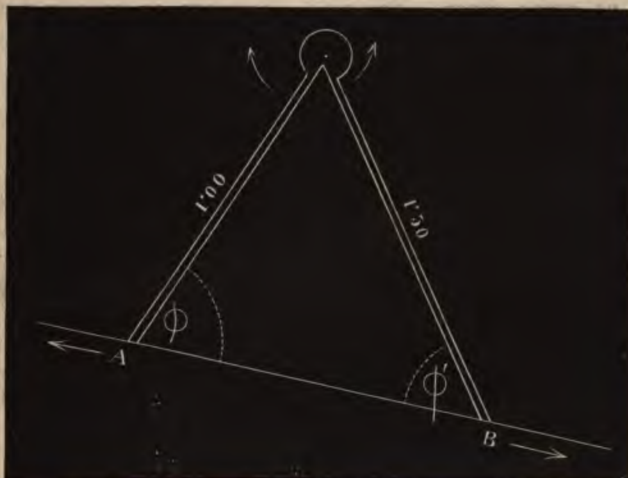


If it be desired to form a perfect square angle of 90°, we have only to form a △ whose sides are in the relation of 5, 4, and 3.*

Again, if a blue ray of light passes out of the spectrum, and is transmitted through two different media, as water and glass, a triangle constructed with its sides representing the refractive indices of the blue ray in each media, and one angle representing

* The square of 5 or 25 being equal to the sum of the squares of 4 and 3, or $25 = 16 + 9$.

the incidence, at the base, the angle of refraction will be represented by the other angle opposite the other side. I may mention one



more fundamental principle. Aberration is at a minimum only when a ray of light passes through a prism at a minimum deviation.

Mr. Browning's very admirable spectroscope, exhibited at the Royal Society, depends upon this principle for its profound and truly astounding powers of spectroscopic analysis. And I do not hesitate to pronounce the opinion that through this subtle instrument alone his name will be handed down to posterity. It is one of the happiest ideas of the practical embodiment of principle.

A prism like a lens forms two foci, primary and secondary; and for the purposes of the most subtle analysis which has ever engaged the human intellect, it is evident the most subtle and perfect powers of differentiation and resolution were absolutely necessary. By a series of prisms automatically arranged so as in each case to obtain the finest definition by means of preserving a minimum aberration, very extraordinary results are now obtained.

The Huyghenian eye-piece was invented on the principle of the angles of immergence and emergence being nearly equal, as in the case of the prismatic *minimum deviation;* and by good luck (most of the best inventions are lucky thoughts) the same principle rendered the eye-piece achromatic.

Now the microscope is really an instrument formed of innumerable prisms. The action of any and every lens can be traced on the prism principle.

There seems, then, good ground for concluding that in all cases the conditions of least aberration should be applied.

A ray of light may be of any colour, and obeys exactly the same *laws* as every other. Spherical aberration is equally true of coloured rays as of light, considered theoretically homogeneous. A violet or a red ray obeys the law of the triangle.

Regard now this rough sketch of gold leaf viewed under a high power by transmitted solar rays. The thing itself is full of mystery and beauty. Why are here red spots, bright spots, and black—*inky black*—edgings? The wave theory of light may give us a clue. Whence comes the rich, translucent, malachite green of the gold? Whence the red and whence the black? Science shakes her head, and says the answer is long and deep. Ask the rainbow and the dewdrop whence come its colours, or the rose-leaf. *Absorption, reflexion, refraction, undulation, vibration* of the *ether* and *rays* of *interference.* Such are the hard dry answers to these simple questions.

But here all definition gets its programme of *sharpness and decision.*

What is this black edging? It is almost specific of a fine definition.

What the red spots? The gold is thicker there, perhaps, but I am not sure of that. The best leaf is only $\frac{1}{150000}$th thick.

Again, view these coarse disks. The same black rings; the light is dead, producing darkness; wave collisions wave.

Sir John Herschel describes some of the effects of interference and diffraction, as producing some of the most gorgeous phenomena in the range of physical science.

But definition is supremely dependent upon the extent, nature, and direction of these collisions and decussations.

It may now be stated in general terms that:—

If an object composed of minute bodies be placed in a blaze of confused decussation, fine definition is impossible.

If there be any confused decussation in the focal rays of the object, as a brilliant point of light, however small, it acquires a spurious exaggerated disk, more or less coloured.

These cardinal facts appear to sum up the whole theory of illumination and definition.

An image of an object is an assemblage of the images of every point in the object.*

If every point produces a spurious disk, the final image is simply an agglomeration of spurious disks: and the definition is better or worse, according as these disks are smaller or larger.

* See Parkinson's 'Optics.'

The spurious disk is that identically the same with the smallest circle of greatest condensation of converging rays.

Here the subjects of illumination and definition obtain a common base of operation. Indeed, the same diagrams and facts which illustrate the one apply equally to the other. (Diagram I., Plate LXVII.)

If conical pencils proceed from Q, and are reflected by a concave mirror obliquely, a series of foci are formed. Confused images are produced of great complexity: and that space or section through which all the reflected pencils pass is a locus of the greatest confusion and decussation.

Each cone of light produces two foci—long and short—primary and secondary. The art of microscopy often consists entirely in selecting the most favourable points of the confused decussation to produce the *desired* effect. I say *desired*, because the microscopist more than any other observer is under temptation to produce special effects, dealing as he often does with the really unknown.

An oblique illumination with a concave mirror is the worst possible source of illumination imaginable from a brilliant point or definite origin of light. Hence the value of the white cloud.

Aplanatic illumination is impossible from an oblique spherical mirror. Indeed, even with the parabolic form of Browning's silver glass mirror the slightest obliquity gives a strong wing or flare to an observed star or *coma*—a sufficient proof of the well-known destruction of aplanatism. The image is no longer clear of aberration or wandering rays. But if we turn to the property of the ellipse the lines Q P, F P, drawn from the foci, are equally inclined to the tangent at P, P R.* Every ray shot from Q against the internal curvature will accurately cross the axis at one and the same point, F, and the two foci are absolutely aplanatic to one another.

In Diagram III. an image is formed by an infinite number of spurious disks corresponding to each point of the object. If a plate stencilled with small apertures in outline be employed, the spurious disk of each will appear expanded, and coalesce into one continuous image, which is very imperfectly defined by a plano-convex lens, when the convex side is turned towards the image. Reversing the lens, the image is improved; if a double convex lens be used, and the image be formed of the same size as the object, by arranging each equidistant from its centre the spurious disks will more nearly equal the true.

I now turn to a novel experiment (at which I feel sure every one that performs it for the first time will be greatly surprised), by which spurious disks of rare and brilliant beauty and significance are developed with the most gorgeous hues. The experiment requires

* Q and F are the foci, and P a point in the curvature.

oblique solar rays from a plane mirror. Having arranged a diatom (the *Pleurosigma Formosum* was the one selected) *with its axis towards the oblique source of light,* and having developed the lines with a good objective, so as to show the structure, cut off the excentrical rays by means of the *Aberrameter.*

Immediately a gorgeous coloured disk can be seen—perhaps four times as large as the individual beading. Now traverse the diatom slowly across the field, lengthways; immediately the brilliant disk appears to roll in a contrary direction, each bead appears in succession (telling a wondrous tale of internal action). With intense vividness of colouring, the rolling disk resembles a setting sun, travelling on the horizon with the motion of the observer in a rapid train; whilst its splendour glows rapidly through all the prismatic colours. Only one disk is visible at once; a new one, with a new colour, starts forth in place of the old at the slightest movement of the object across the microscopic field.

Without commenting upon this magnificent and lovely phenomenon further, than to say that it is evident each bead acts as a refracting sphere, or lens, forming in its focus an image of the sun spuriously enlarged, I pass forward to Diagram III.

Parallel solar rays converge to a focus at F. Opposite the arrows ↕ the rays, in their decussation, pass through the smallest ring. Each ray has here been carefully laid down from the Deviation Tables, refractive index being $\frac{3}{2}$.

In Diagram III. the caustic curve formed by the intersection of the refracted rays, and to which curve they are successively tangents, is shown on an enlarged scale. A spherule $\frac{1}{10000}$th of an inch in diameter, being represented by a segmentary portion, the sphere being delineated 5 feet in diameter.* The short arrows denote the position of the least circle of aberration.

The central rays cut the axis at F, the ultimate, or solar focus —distant $\frac{1}{4}$th of the diameter from the surface of the sphere. Some of the rays are lost by internal reflexion.

It will be seen by examining the diagram that as the aperture is cut off, the smallest ring is found constantly nearer the final focus F. The smallest ring through which pencils from a given aperture can pass is the least circle of aberration; and here it only is reduced to a point where the aperture is exceedingly diminished.

The diameter of the spurious disk of aberration is here about a small fraction of the diameter of the refracting spherule. But in the prismatic effects just described, the spurious disk of an image of the sun was at least four times the size of the bead or lens producing it: producing the very remarkable phenomena already alluded to.

* In the diagram exhibited at the meeting.

If the decussating cones of rays be received upon ground glass or oiled paper, as originating from a distant brilliant point, each position of the screen displays its own peculiarities, which I must not stop to describe, except to notice that the decussation is very intense just beyond the least circle of aberration ⨯ at X *x*.

This may possibly account for the black specific border seen in the fine definition of a brilliant disk.

I wish next to draw the attention of the Society to a greatly magnified representation of a pair of spurious disks formed at the least circle of aberration, corresponding to the arrows in the last diagram. As in telescopes, the disks are smaller as the corrections are more and more refined. The small disk above is the natural size. One diffraction pink annulus, and a black border, completes the picture. (Twelve diagrams were exhibited to the meeting.)

I believe that I have not exhibited these disks to any one of the Fellows, except our Secretary, Mr. Slack (through the microscope). The delicacy required for their exhibition, opposing an immersion $\frac{1}{16}$th to an immersion $\frac{1}{8}$th, so that their foci coincide, a water-film intervening, prevents my showing them with safety on an occasion like the present, as I did at home to Mr. Slack.

I then found, upon viewing these disks, they could be selected in pairs, either *overlapping* or just enlarged into contact. The diameter being known, and their distance apart from centre to centre, the actual expansion of the disk could thus be very conveniently estimated. In this way residuary aberration was abundantly exhibited in the best glasses; 1–16000th became 1–40000th inch.

If now we apply these principles to definition, we see that each bright point has a halo of aberration; if two points be near enough, overlapping images are produced.

Since a line is an assemblage of points, the edge of a fine object, as a scale or diatom, appears sharper, freer from overlapping, as the definition is exalted. If we attempt to define the finest *quill* of a scale, the secondary image or burr universally obscures a pure definition. Whether the straggling rays be represented white, or whether they be coloured, *still* the same want of perfect decisive sharpness, when using high power and large aperture, is more or less to be lamented. Hence astronomers prefer the lowest power practicable to resolve the required details.*

Perfect aplanatism, or perfect freedom from aberration, can only be obtained when all the rays from a point intersect and form

* Mr. Slack has particularly drawn my attention to the appearance of the cracks in fine thin plates of deposited silica. Not knowing the object, I at first imagined from improper focussing that these cracks were cylindrical bodies. But as each crack formed two edges, it had apparently caused two filaments. There appeared, however, two such delusive forms equidistant above and below the best focus. This proved their delusive character.

another point: so that the least circle of aberration is reduced to a point. Considering, therefore, the principles already laid down,—

First, that the cause of inferior definition is oblique spherical illumination, abounding with double aberration in its worst form, it is plain the more perfect we can illuminate by rays free from aberration or decussation, or the more perfect the aplanatism of the illuminator, the more effectively do we destroy the false shadows of decussation.

Again, by aplanatic illumination the light is intensified in the proportion that wandering, decussating rays are concentrated upon the point required, instead of being wastefully dispersed.

If we also wish to develop sharp shadows, the rays spoiling the shadow must be cut off. And when dealing with spherules and molecules, a knowledge of shadow peculiar to them is of the highest importance. According to the form of the illuminating aplanatic pencil, beads can be made to appear shaded into black strokes, crescentic shadows, half shadows, or destitute of shadow. And the optical play of shadow, to the right, the left, above or below, or centrally, when thoroughly recognized, gives the finest possible proof of the existence of spherules, except the brilliant spurious disks already described.

The subject of definition is so inextricably mixed up with the illuminating and image rays, spherical and chromatic aplanatism, or freedom from wandering rays, either white, red, or blue, or any other colour, that the diagrams I now venture to bring before the notice of the Society may be said to illustrate both subjects at once.

Reflexion perhaps is more simple than refraction in a diagram, and therefore let us consider this:—

The points I wish particularly to draw attention to are the primary and secondary focal lines of oblique reflexion (or *refraction*).

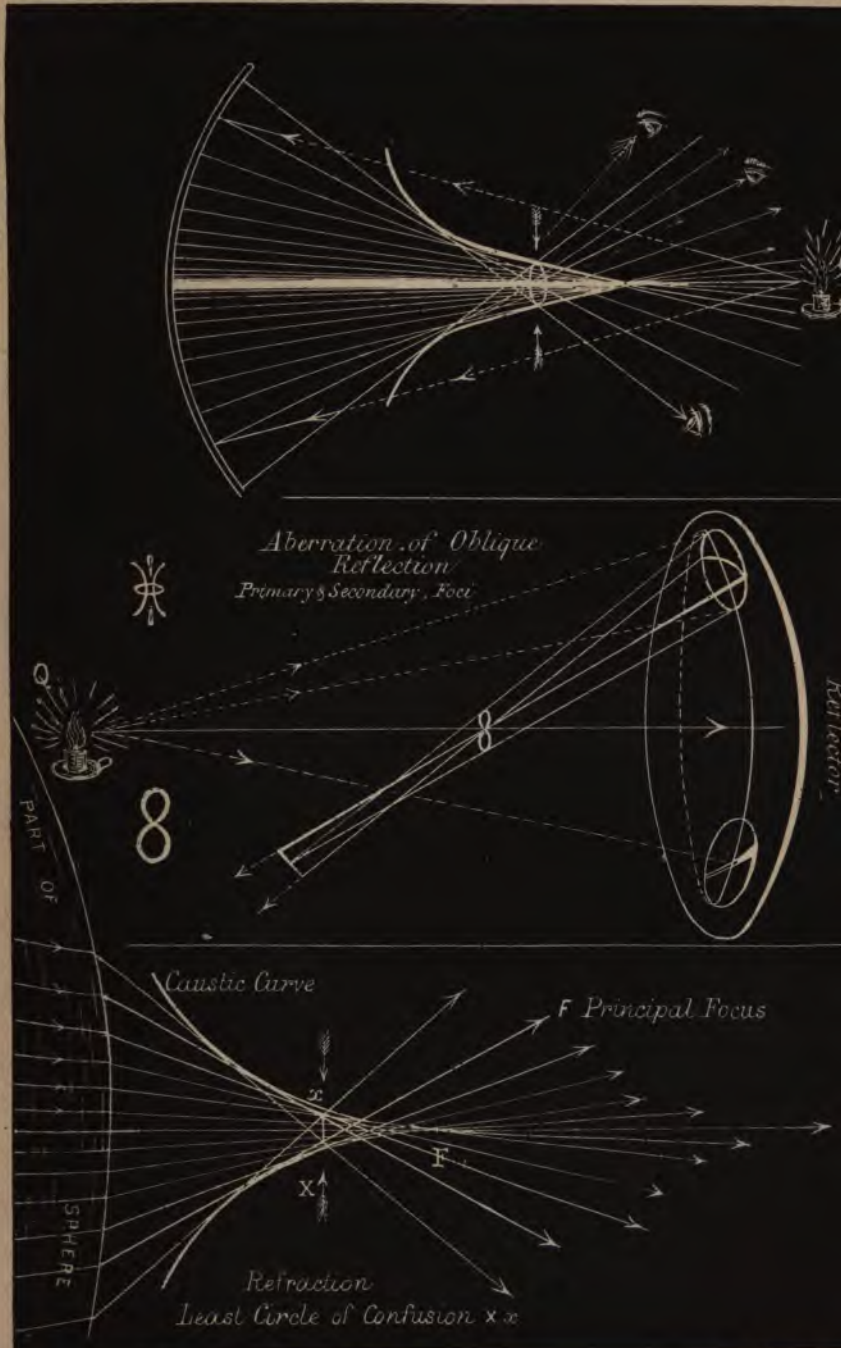
I may state here that even the passage of pencils of rays through the glass cover produces primary and secondary foci. But in achromatic condensers, as usually employed, there are innumerable primary and secondary foci, causing so complex a decussation as to defy all lineal representation. And as in principle they are the same as in reflexion, the same diagram will illustrate it. In Diagram II., Plate LXVII.—Over-corrected pencils. Central pencils focalize nearer the surface, whilst the excentrical cross the axis farther from it. The red rays in a vertical plane cut each other or focalize farther from the surface than the rays in a plane at right angles to it. In consequence of these elaborate longer and shorter foci of given oblique pencils, a figure of 8 is formed at their common intersections; and indeed a variety of figures, instead of clear spurious round disks sharply defined. (Diagram III.)

Practically, in illumination, these are got rid of to some extent

Diagram I.

Diagram II.

Diagram III.



Royston Pigott del

W West & Co Lith

Microscopic Optics.

by stops in the achromatic condenser. But as many lights arranged about a new object disguise its appearance seen from one point of view, so do these incorrigible decussing rays of the ordinary achromatic condenser disguise by confused shadows *real appearance.*

The upper strange figure of a double *fleur de lis* was observed in a French objective of fair quality. The black figure 8 is a common form.

Diagram III. only represents a vertical section: the previous one gives a complex representation of complete pencils.

In Diagram, II. a vertical section of a refracted pencil is delineated converging at a short distance. The rays in a horizontal plane converge to $\downarrow\uparrow$, the locus of greatest light and greatest confusion.

This suggests the propriety of covering up a portion of the concave mirror to improve definition.

The aplanatic definition of the microscope can only be effected when all chromatic and spherical aberration is destroyed by successful compensations.

The aplanatic searcher, by traversing the axis, enables the observer to apply, to some extent, new corrections of a higher order.

The final pencil emanating from the objective becomes more or less convergent at will, the eye-piece remaining fixed, and produces new changes in colour combination and spherical compensation.

It increases magnifying power and penetration, and enables us to use a low objective instead of a much higher one giving as much power.

Deficiency of light is avoided by using aplanatic condensers, which waste none of the light in the diffused blare or burr of aberration—shown readily by the aberrameter on the table. The wasted luminous fog is gathered up to a point, and the light is greatly *intensified.*

The searching lenses found to have the best effect are from $1\frac{1}{4}$ to $\frac{3}{4}$ inch focal length—are formed of two pair of achromatic over-corrected glasses, flint and crown glass cemented traversing the axis, and only slightly changing focal vision, the eye-piece being fixed. Innumerable corrections correlative with the screw collar may be applied and selected by traversing the lenses in search of the finest effects in colour and sharpness.

Frequently an inferior objective has exhibited a better performance under its influence. A Gundlach $\frac{1}{8}$th, lent me by our Secretary, Mr. Slack, would not show the *Angulatum* without it.

The traverse or adjusting movement of the Searcher introduced a new available correction of the objective.

As achromatism theoretically depends not on *form* or curvature, but position and focal lengths of lenses, as well as the refractive indices, the new lenses introduce by their movement a new compensation for different thickness of cover, which, it is well known to first-class object-makers, introduces a disarrangement of achromatism, &c.

There is a certain position, for instance, where the beautiful spherules of *P. Formosum* exhibit, with Powell and Lealand's $\frac{1}{8}$th and Searcher, six black dots round each bead.

The central median beading of Formosum, Angulatum, and Rhomboides can only be seen by aplanatic definition and illumination somewhat obliquely applied.

I may now quote from my first paper, read November, 1869: —"I have found an oblique centrical pencil of aplanatic cones of light of small aperture (15° to 20°) of the greatest practical utility, the obliquity being varied according to the object in view" (p. 297).

This passage essentially describes an aplanatic condenser free from spherical aberration: *i. e.* a first-class objective stopped off to 20° or 15° aperture.

I have been in the habit, for some years, of obtaining aplanatic illumination by means of object-glasses of a fine quality; stopping off such rays as appeared to produce indecisive and confusing shadows. There seems a similarity between the acts of drawing and defining. The masterpiece of the artist, though embodying form, is essentially dependent on a judicious selection and distribution of light and shade. And he only among microscopists who can best manage illumination by the shadow regulation, will best succeed in developing the most difficult structures. The more perfect, sharp, pure, and decisive are the shadows, the greater assistance will be given to a pure definition, banishing as much as possible the useless, the worse than useless, rays producing counter-shadows.

In the early part of the present year I had the pleasure of exhibiting to our President, the Rev. J. B. Reade, F.R.S., Mr. Brooke, F.R.S., Dr. Millar, Mr. Slack, F.G.S., &c., &c., my method of oblique aplanatic illumination, with either a 2-inch, $1\frac{1}{2}$ or 1-inch objective. In order to ascertain that the illuminating pencil is passing centrally through the stop, I place red paper upon the front glass, perforated with an aperture either small and circular or V-shaped, quadrantal, or even semicircular, varying the inclination and position of the pencil by stop-motions designed about six years ago. I had the pleasure of exhibiting the whole mechanical action to Mr. Browning so early as the spring of 1869.

More recently a well-known worker upon microscopic improvement has brought out a similar method of illuminating by means of object-glasses (Dr. Mathews). The smaller and more perfect the *pencil*, the sharper are the s[illegible] of the things to be defined. A

fine black shadow is impossible from decussating rays. If we want to examine any new object perfectly by the unaided sight, the first thing we do is to carefully set it in the most favourable light, be it gem or picture, bust or insect. Can we be exempted from the same pains in getting an *image* of a delicate tissue to be transmitted to the eye?

When in 1862 I first began to study the Podura spherules, I was greatly assisted by first assuming their sphericity, and then trying whether the shadow law applied to them. Could a black shadow be brought out on any side of *Formosum* beads finely developed with a $\frac{1}{16}$th obtained that year (1862)? (The beads were readily seen with the $\frac{1}{8}$th Messrs. Powell and Lealand were so good as to very skilfully construct for me.) This required a rotating stage. I found all the crescentic shadows *rotated* with it. Next, could the Podura bead show them similarly? I found this to be a constant result. Sphericity thus seemed demonstrated. Would blurred shadows entirely obscure or "blot" out the delicate tracery requisite for a fine definition? In telescopes no such difficulties on a clear night are in the way of seeing. A sharp shadow by parallel rays surpasses all others, provided diffraction effects can be prevented in the microscope. Thus the celebrated astronomer Dawes could discern, with his remarkable observing powers, a satellite's shadow on Jupiter, with an 8-inch refractor and a power of 100. Such a point would have been invisible if the outline were blurred off insensibly by a multitude of non-parallel illuminating rays; or even if the satellite were removed a much greater distance from Jupiter. Just as a sunbeam will not cause a sharp shadow of a small sphere, except the shadow is taken on a screen close to it.

I have found fine effects by using a $\frac{1}{2}$-inch objective, with an aperture cut off on the front lens to $\frac{1}{100}$th of an inch, transmitting a sunbeam reflected from a plane mirror. With a power of 4000 and a $\frac{1}{16}$th, I have seen beading in the most remarkable manner in diatoms, &c., when thus illuminated.

It may be interesting to some of the readers of this paper to be informed that my first paper, dated May 21, 1869, and read in December of that year, was to a great extent a transcript of a paper written in 1863, before I had any idea of using intermediate lenses. The drawings of the crowded beading of *Lepidocyrtus curvicollis*, or Test Podura, were obtained with a very fine quarter of Andrew Ross, marked 1851, an eye-piece, C, and about 4 inches of draw-tube. An E eye-piece of Powell's, with the eye-lens or front glass placed in contact with the field glass, and a very fine radial slit for the illumination, and direct lamp-light without reflexion, were the means employed at that time. The original drawing, anastatically printed, has lost its peculiar beauty in copying; but I still adhere to this

valuable record of this interesting observation, though imperfectly represented in the 1869 Plate.

I wish to place upon record my belief—

(1.) That further advance will now be made in the powers of the microscope by improving the lower powers, rather than by the construction of deeper objectives.

(2.) That the microscope will be further improved by using a greater variety of glass, of various refracting and dispersive powers, than hitherto employed.

(3.) By selecting differently dispersive fluids for immersion lenses.

I take occasion to copy from my note-book some results obtained by the camera lucida, and also confirmed by the *Kratometer*,* as regards the amplifying powers obtained with the *Aplanatic Searcher*.

OBJECTIVE "ONE INCH."

	Inches.	Searcher.	Power.
Eye-piece B of focal length ..	2	B″	450
" C " " ..	1	—	900†
" C " " ..	—	without searcher	100

OBJECTIVE "HALF INCH."

	Searcher.	Power.
Eye-piece B ..	P″	700†
" C ..	P″	2100
" B ..	without searcher	260

OBJECTIVE "ONE QUARTER" Ross (1851).

	Searcher.	Power.
Eye-piece B ..	B″	1850
" C ..	without searcher	(about) 400

OBJECTIVE "ONE EIGHTH." (5 *inches of draw-tube.*)

	Searcher.	Power.
Eye-piece B ..	P′	*Bright* 2300†
" B ..	without searcher	1200
" C	eye-piece without searcher 10 inches from object	650

OBJECTIVE $\frac{1}{16}$.

	Searcher.	Power.
Eye-piece B ..	P′	4600 (estimated)
" C ..	without searcher	1600

I have generally found for the very deepest objectives a very low-power eye-piece of 3 inches focus, and a low-power searcher of 1¼-inch focus, placed midway between the eye-piece and objective, productive of the most brilliant results. Having fitted up a great

* To be described in n[illegible]

† Fine definition.

variety of eye-pieces, I may be allowed to recommend the following for the $\frac{1}{8}$th and $\frac{1}{16}$th with *searcher*:—

	Focal length.
Field glass crossed lens	4 inches.
Eye lens crossed lens	$1\frac{1}{2}$ inch.

The aberration of this eye-piece is less than that of many in use.

For parallel rays it becomes achromatic when the interval between the lenses is *two-and-three-quarters of an inch.**

With Powell and Lealand's new $\frac{1}{8}$th, a long draw-tube, searcher and C eye-piece, an extraordinary bright amplification of the Formosum, *estimated* at 4000 diameters; but this was not measured, as the objective was taken away by the makers, having been lent for the occasion.

The favourite power with the $\frac{1}{2}$-inch has been 700; the penetration or depth of focus appeared greatly increased, and many objects were more easily defined with this kind of treatment, especially where great thickness of glass cover was required to be seen through.

Postscript.—The writer regrets that he is unable to introduce sufficient diagrams to illustrate all the points of this imperfect paper.

* Half the sum of their focal lengths.

III.—*On Selecting and Mounting Diatoms.*

By Captain Fred. H. Lang, President of the Reading Microscopical Society.

Up to the present time a well-cleaned but mixed slide of the diatomaceæ from any particular locality has, as a general rule, been considered sufficient; but though such slides are interesting, and in one sense instructive, as showing the prevalent genera and species abounding in a stated place, they are, in another point of view, unsatisfactory, and teach the beginner little. In the very last year of the late Dr. Arnott's life, I heard him say that were he to commence a fresh collection of diatomaceæ he would admit none but perfectly pure and unmixed gatherings into his cabinet. I was much struck with this observation at the time, but reflection convinced me that the longest life would not suffice for such an undertaking, as a pure gathering is the exception, and a mixed one the rule. The systematic diatomist must, therefore, be put to all sorts of straits, and the same gathering has to do duty in the cabinet as an example of various genera and species; and in many instances the particular species to be illustrated, if it is at all a rare one, is almost smothered by the commoner and more numerous ones. This is of course a great drawback to anyone commencing the study of these organisms; as he is confounded with the multiplicity of almost similar forms, and can only with doubt and difficulty identify the one he is searching for.

But if pure gatherings of all species cannot be obtained, it is now comparatively easy to pick out those that are required from the general mass; and it is a matter of wonder to me that this has not been more generally attempted, for I am not aware of a single collection of the kind, with the exception of that which Eulenstein is now making for his subscribers. Selected diatoms of the more showy sort are occasionally to be obtained at the opticians, even arranged in patterns as pretty "Oh law" objects for the microscope; but I would fain induce our members, and others also, to isolate and mount on separate slides every diatom, common or rare, that is to be procured in their own locality, or in gatherings from elsewhere; for if everyone would set to work to do this, we should soon be in the possession of a complete series of really instructive slides of the diatomaceæ.

The object of my paper this evening is not only to insist upon the advantages of this plan, but to render the task easier by giving you the benefit of my own experience, reaped through many failures, in selecting and mounting these exquisitely delicate and beautiful forms.

For a long time I had been content with mounting general gatherings, but having received from Liverpool some Californian shell cleanings, I was fired with the ambition of picking out the

beautiful foreign forms abounding in the general dirt and débris, and, not having the slightest idea how to proceed, I consulted the well-known little work 'On Preparing and Mounting Microscopic Objects,' by Mr. Davis. This, however, did not give the information required. Left, then, to my own resources, I have found that the best implement for picking out diatoms is a finely-pointed badger's hair whipped on to a light handle, so that the hair does not protrude more than $\frac{3}{8}$ths of an inch, thus ensuring sufficient stiffness, whilst undue elasticity is avoided. If this is dipped into a weak solution of gum, and, being allowed to dry, is then breathed upon, the most delicate form from a *dried* gathering may be taken up, and will remain on the hair if it is held steadily, whilst the slide containing the general gathering is shifted, and that on which it is to be placed substituted. On the latter, near the centre, which of course must be marked, has been placed a drop of fluid, to be hereafter described, and into this the hair is lowered, when the diatom adhering to it is immediately freed, and can then be pushed into position in the centre, and thus any number can be arranged as desired. It is preferable to mount all diatoms on the cover and not on the slip; the method of doing this is described farther on. In either case a few minutes' exposure on the hot plate evaporates any moisture, when the collection is ready for the mounting process; and here the beginner encounters two difficulties,—the wave of balsam is apt to float away the diatoms, or should this luckily not be the case, they are often crushed by the contraction of the medium as it dries. Observing that Mr. Davis states that, to avoid the first cause of failure in mounting general gatherings, Professor Ryland adds a drop or two of gum to the last washing of his collection, I availed myself of the hint, modifying the plan to suit my own purpose. Of course, for selection, no gum must be mixed with the last washing, as it is imperative that the diatoms should be perfectly loose and free for lifting; but one or two drops of gum in a quarter of an ounce of distilled water makes a capital fluid, into which the selected forms may be dropped previously to their being pushed into place, and this is amply sufficient to fix them firmly.* A shallow cell formed of paper, card, or tea-lead will obviate the second cause of failure, but this necessitates the trouble and time of papering the slide, whilst Möller's thin-glass cells are difficult to procure, and costly. Here I was for a long time at fault, but happening to make the acquaintance of Captain Haig, who is an adept at mounting diatoms, he kindly showed me that a ring of gold-size, if subjected to great and prolonged heat, becomes perfectly black

* A grain and a half of dry gum-arabic to an ounce of distilled water forms the best solution for this purpose, though for the Campylodisci, Aulacodisci, and some species of Coscinoidisci, &c., which rest on a small portion only of their surfaces, a little more gum is requisite to retain them in their places.

and charred, and is then not affected by balsam, and thus forms the very cell I was searching for. Captain Haig burns these on the hot plate by means of a Bunsen burner, at a great expenditure of gas and much tedious watching, but I find the same result is obtained by giving the prepared slides to my cook to be placed in the oven when she is baking. He uses very thick balsam, but working, as I prefer, with that medium very much thinned with chloroform, it is just possible that even such cells may, in the course of time, be acted upon by it; and so I have since hit upon the plan of forming my rings of gum-dammar dissolved in benzole. Of course I am aware that the chloroform of the balsam will affect these also, but this is of little consequence, as the one material is as transparent as the other, and should they entirely mix, the necessary thickness of medium to prevent crushing is gained. I may here mention that a thin solution of gum-dammar is equally as good as balsam for mounting diatoms; it must, however, be filtered through blotting-paper to get rid of impurities in the gum. For this reason Mr. Tatem, in conjunction with whom I have been carrying on these experiments, objected to the gum-dammar cells, not being aware that it could be so easily filtered, and suggested that they should be formed of a solution of pure gum-arabic, which is not acted upon by balsam. We have both tried such cells, and find they answer admirably. Practically, however, I find it matters little whether the cell is formed of gum-arabic or gum-dammar, and in either case the superfluous portion of the ring beyond the covering glass is easily cleaned off, if desired, in the usual manner, when it would be almost impossible for anyone to tell that a cell had been employed. The pointed end of a small stick (an expended lucifer match will do) is better than a brush to form these ring-like cells, as it can be thrown away and a fresh one used on each occasion.

Some delicate diatoms are almost obliterated in balsam, and should be mounted dry. This is not such an easy task as it may appear, as such slides are apt to become spoiled by the penetration of damp, which eventually forms in beads of moisture on the inner surface of the covering glass. Captain Haig for such purposes still uses his gold-size rings, which in this case are not completely charred, but are removed from the hot plate, whilst the point of a needle will still leave its impression on the ring as long as it is hot, but will not do so when it cools. By replacing the slide on the hot plate at the moment of mounting, the ring is once more softened, and the cover applied and pressed down to ensure adhesion. I have, however, found that the fumes from the heated gold-size are liable to form crystals, which are as unsightly, if not so injurious, as the beads of moisture; and so I prefer using his charred ring, which I smear with my gum-dammar solution, on which, whilst it is still slightly viscid, I press down the covering glass; and afterwards, if the adhesion has been mad nd the whole periphery, a

finishing coat of asphalte or other varnish may be added, and thus all danger of damp or of crystals is avoided.

Captain Haig gave me two other most useful hints, which he has most kindly allowed me to mention. Of course all diatoms should be mounted on the cover. To secure the correct centering of them, he forms on a glass slip, by means of the turn-table, a ring of gold-size $\frac{5}{8}$ths of an inch in diameter, the size of his covering glass, and within this a very minute one exactly in the centre. This is hardened by heat, as his cells are. On the outer ring, at equal distances, are placed three little bits of beeswax. The covering glass, on which it is intended to arrange the diatoms, is placed on this general mounting slip, and slightly pressed on the wax. Instead of distilled water, he places on the cover a very slight smear of glycerine, into which previously, as in the case of the water, a drop of gum may have been added. Into this the diatoms are dropped, and may then be pushed within the inner ring, and their perfect arrangement and centering are secured. The advantage of the glycerine over the water is that it is a greater solvent for freeing the diatoms from any extraneous dirt, and that it will remain moist for any length of time. When the arrangement is completed, the covering glass is gently pushed off the three pieces of wax, and transferred from the slide to the hot plate, when in a few minutes the glycerine is evaporated. Put on another slide under the microscope, a drop of benzole is placed on the diatoms, and whilst they are being permeated by it, and all air displaced, a little balsam is dropped into the prepared cell, when the cover is seized by the forceps, reversed, and placed carefully on it, and the mounting is completed in less time than it has taken to tell the process.

I must state, however, that Mr. Tatem, whose slides of diatoms are almost absolutely perfect as to cleanliness and neatness of arrangement, prefers simple distilled water, with of course the necessary addition of gum, to the glycerine, which he considers messy. He uses a finely-pointed sable brush, instead of the badger's hair, both for selecting and arranging; for though, owing to the rapid evaporation of the water, the diatoms may become dry and fixed before they are exactly in position, a touch of the brush again moistens them, when they may be shifted as required. He also places his cover with the diatoms on the cell, which has been simply filled with chloroform, and then, placing the balsam at the edge, allows it to run in and replace the more volatile medium as it evaporates, thereby avoiding all chance of an entangled air-bubble. This process, however, must entail a good deal more trouble in cleaning off the superfluous balsam; and after mounting many dozens of slides after my plan I may fairly assert that I have never once been troubled by a persistent air-bubble; whilst a very little experience teaches the manipulator the exact quantity of balsam

required for such very small cells, so that no after-cleaning is necessary, with the exception of washing the upper surface of the covering glass with a little soda and water. This, it may be remarked, does not affect the gum-dammar ring though it would the gum-arabic one, nor the balsam in which the diatoms are mounted; whereas if spirit is used to clean off the slide, considerable danger is incurred lest the cover be shifted, whatever may be the nature of the cell, using as we do very fluid balsam.

Of course for the selection of the smaller diatoms a higher power than is afforded by a simple lens is requisite. Some persons can work with the compound microscope though every movement is reversed by it; but for those who cannot do so, some kind of erecting microscope is necessary. Mr. Curteis, of Mr. Baker's firm, has kindly arranged for me such an instrument. It consists of his "travelling or sea-side microscope," to which has been adapted a French erecting prism over the eye-piece, whilst above the circular glass stage is firmly fixed an oblong wooden one, giving sufficient rest for the hands and wrists of the manipulator. I cannot speak too highly of its practical use for the purposes I require. When thus employed as a dissecting microscope, the stage is in a horizontal, and the body in a vertical position, with the prism at almost a right angle; and thus the workman sits in a natural, unconstrained position, looking straight to his front, and the tedious and unhealthy stooping position entailed by the simple dissecting microscope is avoided. It is at the same time in its ordinary state, without the prism and extra stage, a very handy and useful little instrument for the purposes it is intended for.

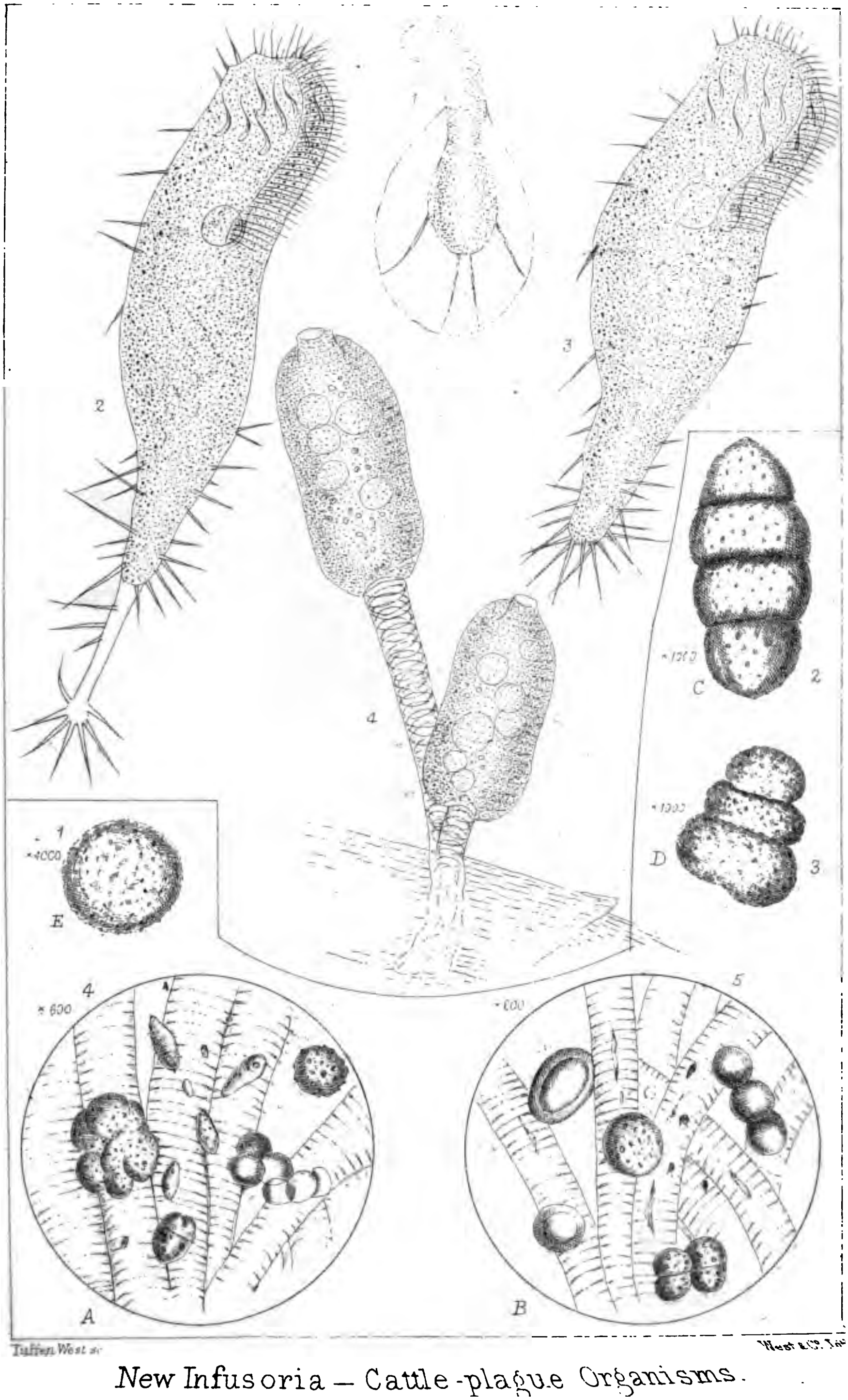
In conclusion, I will only remark that though perhaps none of us can hope to equal Möller's beautiful type-slide with which we are all so well acquainted, I do trust that the time is not far distant when the diatomists of this country will be able, each of them, to make type-slides of the prevailing species of their own localities, arranged according to some acknowledged synopsis; for thus alone can a sound knowledge of these organisms be obtained, the vast number of synonyms be reduced, order replace the present chaotic confusion, and the pursuit converted into a regular science.

IV.—*On Certain Cattle Plague Organisms.*

By Boyd Moss, F.R.C.S.

Plate LXVIII. Lower portion.

While engaged, as a member of the Ceylon Cattle-Plague Commission, in searching for the entozoa described by Dr. Lionel Beale as existing in the muscles of the animals dead from the disease, I *discovered* the very remarkable bodies which are represented in



New Infusoria — Cattle-plague Organisms.

the accompanying drawings. Whether they are ova or spores, or whether they have any connection with Dr. Beale's entozoa, I have not been able to ascertain. The latter are by no means common in animals which I have examined in Ceylon; and only in one instance have I found these bodies and Dr. Beale's entozoon together in one specimen. They seem to lie loose among the muscular fibres of the heart; sometimes in large numbers together, at other times singly. Their great variety of form is very remarkable. Their minute size is shown in the scale to which they are drawn. Figs. C, D, E, are carefully copied with the camera lucida, and a power of 1000 diameters. The spherical body E is perhaps more common than the other forms, and is always of a golden brown colour. The others are sometimes of a dark olive brown. I have examined the parts of numerous healthy animals, and only in one instance (in the heart of a Sambur deer) have I found any of these bodies. In the hearts of three animals which died of murrain in one locality they were very plentiful. One of these was a cow in calf, and some were seen even in the heart of the fœtal calf. I may mention by-the-by that the specimens from these three were examined on the day of death.

There is a drawing in the 'Micrographic Dictionary' of the spore of *Stilbospora macrosperma*, which has some slight resemblance to my Fig. C. Nearly all these bodies, however various in form, agree in the dotted appearance of their surface. I hope that some reader of the 'Microscopical Journal' may be able to throw some light on the nature of these.

CEYLON, *Sept.* 6, 1870.

V.—*Notes on New Infusoria.* By J. G. TATEM.

PLATE LXVIII. Upper portion.

OF the Infusoria, of which camera drawings are laid before you, I purpose giving such brief descriptions as may be necessary for their explanation.

I.—Fig. 1. A Difflugia of small size—$\frac{1}{300}$th—with smooth, very transparent chitinous test, flattened beneath, rounded above, with circular, smooth, direct aperture, through which the sarcode flows in a feeble undivided stream, containing very minute, greyish granules. From the posterior part of the bottle-shaped body four filamentary diverging ligatures stretch to the fundus of the test, binding down and securing the animal in its suspended position. These filaments—extensions of the pellicular investment—have, I think, a peculiar interest, not only as the sole example known to me of such a structure among the fresh-water *Rhizopoda*, but as demonstrating a fact in the *Difflugiæ* at least, as much disputed as admitted

in the cognate naked *Amœbæ* of such a surface differentiation from the internal fluid sarcode, as may unquestionably be considered as distinctly cuticular.

For this form of *Difflugia*, I propose the specific name of *ligata*.

II.—The *Stylonychia*, Figs. 2 and 3, an aberrant form of *S. pustulata*, is a remarkable one; the rounded posterior end of the normal *S. pustulata* being in this example produced into a hyaline, extensile process or tail, of about one-third the entire length of the body, having a few stout setæ scattered on its upper edge and terminating in a palmate expansion fringed with spines. This tail is projected by the animal in a state of rest, but is instantaneously withdrawn when in motion. These extensions and retractions are attended by, and consequent on, changes in the outline of the body, which is elongated and narrowed in the one condition, shortened and broadened in the other (Fig. 3), its compression or expansion causing a propulsion of semi-fluid sarcode into, or a reflux from the caudal appendage, and thus effecting its protrusion or contraction.

III.—Of the *Vorticellid* (Fig. 4) I can only speak with hesitation and doubt. In no one of the half-dozen specimens met with, either through its own sensitiveness, as shown by the ready separation of the zooids from the stalk, or the restless movements of its host, have I had the opportunity of observing the expanded peristome and ciliary disc, though both are clearly discernible in their retracted condition. In that state they closely resemble those of an *Epistylis*, with which its general character suggests its association, and to that genus I shall venture to refer it, though presenting a wide divergence from the *Epistylid* type of ramification, in bearing its zooids at unequal heights. How far deviation from rule in this respect should operate in excluding it from the genus *Epistylis* I must leave, however, to more competent authority to determine.

Its specific characters are as follows:—

Epistylis ———? (Fig. 4). Zooids, two—one terminal, the second borne on a short stalk springing from below the middle third of main stem, similar, large—$\frac{1}{250}$th—elongate elliptic, faintly transverse striate, colourless. Peristome and ciliary disc unknown, when retracted forming a mammillary projection at the summit of the zooid, contractile and several large food vesicles observed, but no nucleus. Stalk beautifully transparent, strongly and somewhat closely corrugated to near the base, where it is flattened and expanded with a few longitudinal striations.

Parasitic on *Cyclops quadricornis*.

READING MICROSCOPICAL SOCIETY, *November* 1.

NEW BOOKS, WITH SHORT NOTICES.

A Handbook of Medical Microscopy. By Joseph G. Richardson, M.D., Microscopist to the Pennsylvania Hospital, Philadelphia. Lippincott, 1871.—It is a curious fact, but not the less a true one, that almost all books which America produces, however good they may be, are yet considerably below their fellows in this country. Of medical and scientific manuals this is especially true, and we are compelled to consider Dr. Richardson's volume as one of the class to which we refer. This inferiority is especially true in the case of scientific manuals, which, however well they may be got up, and however fully they may be illustrated, are yet far behind their European fellows. Yet, perhaps, as there is no exact equivalent of Dr. Richardson's volume in this country, his may prove somewhat exceptional. It is a work addressed almost wholly to medical men, and it treats at first of the microscope in its different forms, and then of the several apparatus which are required in order to work with it. But on looking through its pages, we find little that is not to be found, even on the medical matters, in such volumes as Carpenter's, Beale's, and Hogg's; while in reference to the mechanical features of the microscope it is very sadly deficient. Furthermore, it only contains about thirty woodcuts, while any of the English works, Carpenter's for instance, including the plates, has about 500; and this is a deficiency which we cannot but note in a microscopical work, in which, above all, the reader—particularly if he be a novice—requires to be instructed by the aid of good illustrations.

Having said a good deal that is against the author, we may add a word or two in his favour. He has evidently considerable experience in the various forms of microscopic apparatus, and he has himself a great name as an American worker; and it is these facts which make us speak so severely of his book. But he has in the manipulation department of his work given us the fruits of his research, and he has been at pains to bring down his matter to as late a date as possible, events of the last year finding a constant place in his volume. He seems to support the dry lenses, though he gives favourable mention to those of the immersion system, which, however, he does not appear to adopt. In all respects, so far as we can see, the author has put together in a terse form the views of the several Continental and English observers on microscopic subjects; this part of his work being immensely beyond the other in the manner in which it is executed.

On the whole, we imagine the work is suitable to the medical student; but we think when it comes to its next edition that the author will do well to attend to some of our remarks, and extend at least that portion of his volume which relates to microscopical apparatus.

PROGRESS OF MICROSCOPICAL SCIENCE.

The New Sponges.—Readers of Mr. Kent's paper in our Journal will find Dr. Oscar Schmidt's new work full of interesting details anent the sponges found in his recent dredgings. Some of his drawings are indeed beautiful. The work is almost folio in point of size, and is illustrated by six plates, which are unquestionably superior to almost anything done in this country. The memoir should be carefully read by those who are interested in the subject.

Change of Structure in the Bones.—Dr. H. E. Sauvage has published a memoir which is analyzed at length in the 'Journal de l'Anatomie,' No. 4. He has taken certain of the bones of the head, and examining them at different ages, from the fœtus upwards, he points out and figures the different changes which he believes are recognizable by the microscope.

The Development of the Egg.—A somewhat important review of recent writings on this point, including Waldeyer's, Van Beneden's, His', Van Bambeke's, and others, and which is of some length (about 28 pp.), is to be found in the 'Archives des Sciences,' No. 152. It is a review of the several writings on the subject of egg development, &c., for the past three or four years.

Function of the Sympathetic System of Nerves.—We must apologize to both the author and our readers for not earlier noticing this paper. It was read before the Royal Medical and Chirurgical Society in June last. It contains some important novelties, and deals with minuteness with the structure of the sympathetic ganglia. The author holds that every sympathetic ganglion is in all animals constructed with three forms of nerve-fibres—motor, sensory, and sympathetic proper. It is a paper of essential interest, and is by Dr. Edward Meryon.

Pathological Changes of the Muscles.—M. Georges Hayem continues his series of papers on this subject in the 'Archives de Physiologie' for 1870, No. 4. He does not conclude his observations in this No., but goes on and deals with those changes which follow severe cases of small-pox—those, in fact, in which death quickly occurs. A well-drawn plate accompanies his paper.

Distribution of Nerves in Electric Organ of Torpedo [*T. Narke*].—Professor G. V. Ciaccio gives a paper, in which he sums up the conclusions at which he has arrived.—*Archivio par la Zoologia*, &c., Ser. II., Vol. II.

Experiments on Spermatozoa.—In the above journal Professor Ciaccio also records some experiments on Spermatozoa with hydrogen and other substances. They are merely worthy of mention however.

A New Species of the Genus Bomolchus.—This is minutely described and figured in a paper by Professor Sebastiano Richardi, in the above Italian journal.

Two New Species of Macrostomum are described by M. Edouard Van Beneden in the 'Bull. de l'Acad. Royale des Sciences' for 1870, No. 8. He calls the first *M. Viride*, and another *Omalostomum Claparedii*,

and gives an excellent drawing showing the whole organization of the first.

Results of the Injection of Parasitic Spores into the Blood of Animals.—The interesting discoveries of Hallier, Zürn, Chauveau, and Davaine, that there is contained in the blood in several contagious diseases numerous parasitic growths, known as micrococcus, mycothrix, leptothrix, &c., have stimulated further investigations in this comparatively new field, among the more recent of which are those of E. Semmer, Prosector at the Veterinary Institute at Dorpat, reported at length in a recent number of Virchow's 'Archiv.' His experiments consisted first in the repeated examinations of the blood of animals which had died of contagious diseases, with the view of detecting therein any parasitic growths, and his results may be briefly summed up as follows:—In cases of glanders, micrococcus-cells and bacteridiæ are found in the blood, lymph, and pus, in animals which have died of this disease. In charbon (malignant vesicle) and septicæmia, the blood is found to contain numerous micrococcus-cells, and bacteridiæ (mycothrix and leptothrix), both in the form of simple filaments and also arranged together so as to form a chain, the latter growths having been long known under different names. That these infusoria, having the form of slender cylindrical filaments, really are parasitic growths, was proved in the case of five pigs, which had died of septicæmia. In the blood of these animals, the actual growth of the filaments from the micrococcus-cells was demonstrated under the microscope. The bacteridiæ seen in cases of charbon are, for the most part, shorter, more slender, and have a less distinct outline, than those found in septicæmia, where they vary in length, and consist of a single well-defined filament, and are, at the same time, less numerous than in the former disease. These filaments, however, are not always of the same form, even in charbon, for here also variations are noticed, so that at times the parasitic growths in this disease resemble, both in form and size, those found in septicæmia. These parasites, moreover, are found in other diseases characterized by blood-poisoning, and even in the blood of some healthy animals a few micrococcus-cells (*Penicillium glaucum*) have been found, and in other cases both leptothrix and micrococcus (bacteridiæ) have been seen in the liver and intestines. In order next to determine whether the contagious diseases above referred to are really caused by the parasites found in the blood, the author has tried a series of experiments, with the following results: Penicillium-spores, of the size of a red-blood corpuscle, were mixed with distilled water, and then injected by means of a small subcutaneous syringe into the jugular veins of two colts. The animals remained perfectly well, however, after the injection. Several weeks later micrococcus-cells were injected into the same animals, and here again the injections were not followed by any symptoms of disease. Injections of anthrococcus-cells were likewise followed by negative results. A second series of injections was next made with large masses of the spores of *Penicillium glaucum*. In these cases slight feverish symptoms were created, which soon disappeared, however, and, upon an examination of the animals after they had been killed, all the organs and textures of the body were found to be quite normal. The author next injected

bacteridiæ from the blood of an animal affected with charbon, using, in this instance, a bottle with a glass tube attached, as recommended by Hallier. The spores of this parasite were put into distilled water, and a few drops of this mixture were then forced into the jugular vein of a colt. At the end of five days the animal remained, to all appearance, quite well. Five days after the first injection, two ounces of the same fluid, containing bacteridiæ and micrococcus-cells, were thrown into the jugular vein of the same animal, by means of a large syringe, armed with a small needle-like canula. This was followed by symptoms of fever, without loss of appetite, and the animal appeared soon to recover. On the tenth day after the inoculation, however, fever again set in, the appetite failed, the animal exhibited an unsteady gait, and died during the night.

The following was the result of the autopsy:

In the neck, around those spots where incisions had been made for the purpose of injections, the connective tissue was thickened and infiltrated with serum, exhibiting here and there ecchymosed patches. The jugular vein was filled with a dark-coloured blood of the consistence of tar, but otherwise not changed. Lower down on the neck there was well-marked serous infiltration in parts remote from the point of injection. On the left shoulder there was extensive extravasation of blood beneath the superficial integument, involving the connective tissue, and extending down beneath the muscles. In the abdominal and thoracic cavities and pericardium slight serous exudation. Numerous ecchymosed patches in the peritoneum, intestines, pleuræ, heart, and lungs. Spleen enlarged, friable, and gorged with blood. Liver of a yellowish-brown colour, and the microscope showed beginning fatty degeneration in this organ. Kidneys studded with large, yellow, hardened and congested masses; while fatty degeneration of the epithelial cells of the uriniferous tubules communicated a yellowish-grey colour to these entire organs. Lymphatic glands enlarged, softened, and several were of a brownish-red colour, caused by extravasation of blood. Brain and spinal cord infiltrated with serum, while the minute blood-vessels were distended with blood. Ventricles filled with a clear, colourless liquid. The blood was everywhere of a dark-brown colour, of the consistence of tar, and contained large numbers of parasitic growths characteristic of charbon.

Here, then, charbon was produced in a colt by the injection of parasites taken from the blood of animals affected with that disease. In this instance the symptoms broke out on the ninth day after the injection, death occurring on the tenth day.

Presence of Microscopic Bodies in Water.—In reference to this matter, Mr. C. Stodder has communicated a note to the 'Boston Journal of Chemistry' for August. Referring to Mr. Metcalfe Johnson's views [in these pages], he says:—"Having undertaken within a few months to ascertain the organic contents of the air in Boston, he commenced by drawing a large quantity of air through a small quantity of filtered water, which, when examined with the microscope, appeared microscopically pure. He found large quantities of organic matter, a very few organic f t many very minute bodies,

which, may be, were fragments of organic or inorganic matter, or monads, or germs, according to the fancy or theory of the observer. Not feeling quite satisfied as to the purity of the water used, he evaporated a drop of it on glass. Placing that under the microscope, he beheld at once a collection of the same organic matter and minute bodies that he first supposed he had obtained from the air. This put an end to the use of filtered water. He then obtained from a friend a sample of carefully distilled water, which had been, however, distilled a month. Testing that in the same way, he found the same things more abundant even than in the filtered water. Another sample, also distilled some weeks, but kept with great care by Prof. J. Bacon, of Boston, U.S., gave precisely the same results. A third trial—on a sample freshly distilled, expressly for him, from a perfect apparatus, entirely copper and tin, the steam nowhere coming in contact with glass—also gave organic fragments, looking like degenerated or decaying epithelial cells, as before, and vast numbers of the minute bodies, $\frac{1}{50000}$th or $\frac{1}{10000}$th of an inch in diameter. Although nothing could be seen by the eye in the water in a bottle (a bottle has considerable magnifying power on its fluid contents), yet there was sufficient deposit from one drop on the glass slide to be distinctly visible. These results indicate that no reliance is to be placed on any conclusions or inferences to be drawn from any experiments yet reported, as to the existence of organic 'germs' in the atmosphere, where the medium of collection or examination was water; and when glycerine is used, we cannot follow any development of the supposed germs to ascertain their nature. The plan proposed by Dr. R. L. Maddox, in the June number of the 'Monthly Microscopical Journal,' seems to be the most reliable, but even that is not absolutely certain. The question now is, is it possible to obtain in any manner absolutely (optically—not chemically) pure water?"

Infusorial Deposits in America.—A paper was lately read (May 16) before the New York Lyceum of Natural History, "On the Ancient Lakes of Western America: their Deposits and Drainage." After the conclusion, Professor A. M. Edwards made some remarks on the microscopical examination of the fresh-water Infusorial deposits of the West, and classification of these and the marine strata, containing microscopical organisms which he had adopted. Besides the recent deposits, made up for the most part of muds of both marine and fresh-water origin, we have Lacustrine sedimentary deposits, now forming in lakes and ponds, and which belong to the present or Post Tertiary Period; the Sub-Plutonic or lake deposits of the West, only found as yet in that part of our continent where volcanic action has prevailed, and the marine deposits of the coast range in California, Virginia, and Maryland on the Atlantic coast, Japan, Payta in Peru, Moron in Spain, the island of Jutland in Denmark, and the islands of Trinidad and Barbadoes, all of which were Miocene Tertiary. A full exposition of this subject would be published in a future volume of the California Geological Survey, upon which he was now engaged.

NOTES AND MEMORANDA.

Mr. Browning's Spectroscope Tables.—Mr. John Browning has after considerable difficulties succeeded in preparing a number of tables of spectroscopic spectra at an exceedingly low price, and which cannot fail to be of immense value in working with the spectroscope. Each table is about the size of one of the plates in this Journal, and contains no less than seven spectral bands, each measuring nearly three inches across. At the top of the plate are indicated the principal lines in the band by means of the well-known letters; and the plates are so constructed that either a pen or a pencil may be used to mark down the bands obtained by experiment. The price we believe is extremely low.

Examination of Fibres in Mixed Fabrics.—In the 'Chemical News' of October 7th, Mr. John Spiller gives a long account of the various chemical processes used for the purpose of helping one to disconnect the various forms of fibres. It is essentially a paper dealing with the chemical tests. For the microscopical methods he refers to Mr. Suffolk's recent treatise.

Vitality of Beer-yeast and Vaccine Lymph.—M. Melsens, who has contributed a memoir on the above subject to the 'Bulletin de l'Académie Royale des Sciences de Belgique,' No. 7, 1870, says that, as regards beer-yeast, his former experiments have been confirmed. With regard to the vaccine lymph, he found that it maintained its vitality even when exposed to cold, artificially produced, of 80°.

Diseased Milk.—In cattle which are infected by the foot-and-mouth disease, it seems that the milk rapidly becomes infected by microscopic fungi. The 'Medical Press and Circular,' which has a note on the subject in its issue of September 21st, gives the following facts:—In one farmer's house every member of the family suffered from *parasitic thrush;* they were consuming milk from diseased cattle, and the medical attendant ascribed the existence of *oidium albicans* to some *materies morbi* existing in the milk, for when the use of it was discontinued the mucous surfaces affected speedily recovered their normal state. So it has been noticed by others that when suspected milk was used and *aphthæ* were produced, that a discontinuance of the milk soon permitted the disease to yield to simple treatment.

American Association for the Advancement of Science.—This Association closed its nineteenth meeting on the 25th of August, at Troy, New York. Owing to the illness of President William Chauvenet, of St. Louis, the Vice-President, Dr. T. Sterry Hunt, of Montreal, presided. The meeting was largely attended, there being about three hundred names enrolled on the Treasurer's books. The next meeting is to be held in Indianapolis, and the meeting in 1872 will probably be in San Francisco, upon invitation of the California Academy of Sciences. The President-elect for the next meeting is

Professor Asa Gray, of Cambridge; General Secretary, Professor F. W. Putnam, of Salem, Mass.; Treasurer, Mr. William S. Vaux, of Philadelphia.

Pritchard's Infusoria.—At a late meeting of the New York Lyceum of Natural History, Professor T. Egleston, jun., read a letter received from Dr. Eulenstein, of Berlin, speaking of his forthcoming revised edition of 'Pritchard's Infusoria,' and asking for contributions of specimens for the purpose of furthering that undertaking.

Mr. Lee's Microscopic Evening at Croydon.—Mr. Lee gave the soirée of the Croydon Microscopical Society on the 24th of November. It was all that could be desired, both in guests and objects of interest. We are sorry that the lateness of the month prevents our noticing it properly, but in our next number we shall give a full account of the conversazione of this young and successful Society.

PROCEEDINGS OF SOCIETIES.*

Royal Microscopical Society.

King's College, *November* 9, 1870.

Dr. Millar in the chair.

The minutes of the last meeting were read and confirmed.

A list of donations was read, and a vote of thanks passed to the respective donors.

Notice was given that Dr. Lawson would at the next meeting move, "that the meetings of the Society for the next year be held on the first, instead of the second Wednesday in each month of the session." The question came before the Council last year, and a motion was made thereon; but as it was found that the arrangement, while being convenient to some would be equally inconvenient to others, the motion was withdrawn. Such strong representations, however, had been made by Dr. Lawson, and Mr. Hardwicke, the publisher, as to the detriment which the Journal would suffer if the meetings could not be held earlier in the month, and thus allow matter for publication to be sent to the Editor at an earlier date, that the Council have decided to give the subject further consideration, with a view to advise the next meeting.

Mr. S. McIntire read a paper "On Notes on the Minute Structure of Insect Scales." Dr. Royston-Pigott said he was much pleased with Mr. McIntire's paper. He thought it had opened up a new field of investigation for microscopists for some time to come. It was impossible for him, however, on that occasion to follow all the details

* Secretaries of Societies will greatly oblige us by writing their reports legibly —especially by printing the technical terms thus: H y d r a—and by "underlining" words, such as specific names, which must be printed in italics. They will thus secure accuracy and enhance the value of their proceedings.—Ed. M. M. J.

into which the writer of that paper entered; he would therefore make a few remarks upon one or two points on which Mr. McIntire had touched; and first as regards the Aplanatic Searcher. He did not mean to take any great credit to himself for its discovery; but if they knew the history of its origin, and the great variety of lenses and object-glasses inserted intermediately for above six years between the eye-piece and the objective, they would still think it capable of improvement. He would mention a similar instance of intermediate lenses. Professor Listing professes to get, by means of such lenses intermediately applied, a diameter of 17,000, or it may be 50,000, and to obtain a definition of *Angulatum* in a most marvellous way. The Barlow lens is also an instance of an achromatic lens placed intermediately between the eye-piece and the objective, but forming a virtual image. If this lens be applied to the telescope, it amplifies the image about one-half without interfering greatly with the definition. But if tested on severe objects, as brilliant double points of light, those points appear to be brought closer by a spurious disk enlargement.

Dr. Pigott then alluded to the opinions which some years ago prevailed as to the structure of the Podura scale, and stated that some eighteen years since, he himself had viewed the scale as being "hackled" all over with spikes, which Mr. Powell showed distinctly to him in 1852 with his new $\frac{1}{8}$th. Then with regard to definition; before anyone could speak of perfect definition, he must first of all be sure that he could define a point, because every image that is seen through the microscope may be considered as an assemblage of such points, and therefore the attainment of perfect definition of a point free from the false penumbra of the secondary spectrum would be a solution of the whole question. The primary question which every microscopist should ask himself is, Can I define a point? If he could do that, then he could define an assemblage of points. If he could not, a false image was the result. Mr. McIntire had alluded to the markings on *Lepidocyrtus*, which he (Mr. McIntire) considered to be square in form. Now twenty years ago, the markings on the Podura scale then seen, were thought to be "ghosts," and in all probability these "ghosts" are but the shadows cast by the real objects. Dr. Pigott then described by means of a diagram the false appearances produced by the crossing of two strata of spun glass $\frac{1}{3000}$th of an inch in diameter, which when placed under the microscope would constantly vary as the focus and their inclination, or angles of intersection were changed. He also alluded to the conflicting opinions of microscopists as to the proper focussing, object-glasses, &c., as regards the particular plane preferred. He believed that whatever is seen through a false medium prepared by human art, such as a compound microscope, is commingled with human error, for it was impossible that any work performed by the hand of man should be absolutely free from imperfection like the works of Nature; and he admitted this principle into all the investigations he had made. Instead of depending upon the appearances of unknown objects for tests, he simply depended on the appearance of known objects; thus, referring to the effects produced by two super-

imposed strata of the cylinders of spun glass, he would let it be assumed for the occasion that the Podura scale was really formed of cylindrical or semi-cylindrical corrugations; it did not matter whether there were two membranes or only one. Wherever these cylinders intersected they would develop the waviness which produced the black markings so often referred to; and with a low power the undulations were to be seen, as he himself had seen them twenty-five years ago with a ½-inch on the Podura scale, and at that time thought them wonderful. By putting on more power a variety of effects would be obtained. The intersections of the hairs of the antennæ of the male gnat were similar in effect, and produced a beautiful test. Looking at the fact that these effects were dependent on three or four conditions; on the symmetrical equality of the corrections at points on both sides and equidistant from the focal point; on the variation of these corrections; on over-correction or under-correction of the object-glass; could anyone predict what would be seen in an unknown object? The practical point was whether the observer could define a point; for the false images above and below the focus when an object consisted of two strata only mystified, and indeed often misled him, to select that which was false because more captivating than the true one. If three observers, A, B, C, were each exhibiting a different object, in all probability each would declare his own method of focussing the favourite appearance was the right one; yet all being different, which would be the right one? He appealed to photography in confirmation of the principle of judging of definition by comparing a known object with its image, for the photographer was never satisfied until he could develop an image true to nature. Then only could the definition be ascertained. He thought they were only at the very beginning of this matter; and regarding the progress that had been made during the last thirty years, he looked forward to a more than corresponding improvement in the future powers of the microscope in the course of a future generation. No sane person would venture to deny the probability of such an advancement being made hereafter.

Mr. Slack observed that Mr. McIntire confirmed the existence of beads in some cases, and it seemed to him that analogy intimated their reality in others, where without such aid the optical difficulties would lead to hesitation and doubt. *Lepidoptera* and *Thysanura* scales seemed constructed upon the same plan of two membranes with corrugations and dotted deposits between them. He did not like calling these deposits pigments, because in many cases they were colourless, or nearly so.

With regard to the scales of *Urania Leilus* referred to by Mr. McIntire, he found it beaded in all positions when the scale was rotated and the illumination was unilateral from a single radial slot in a condenser. Observations made with torn scales showed that when no crushing occurred, the severance took place between the bead rows, the ends of which exhibited distinct terminal beads. If such scales as those of *Morpho Menelaus* were softened in a drop of warm caustic soda solution, and then torn by rubbing a covering glass over them, the same appearances would be seen. In his examinations of *Lepidocyrtus*

curvicollis scales with the new and magnificent $\frac{1}{8}$th of Powell and Lealand he had seen the beads mentioned by Dr. Pigott.

He wished to make one remark about Dr. Pigott's frequent reference to points. A mathematical point had position without magnitude; but no image, however small, was a point of this sort. Optically speaking, he took a point to be the smallest portion of space which will comprehend the waves of the smallest light-pencil we can see.

Dr. Pigott briefly replied, explaining that he meant in his remarks not a mathematical point, but the smallest visible point.

Mr. J. Beck said that his late brother, in his work on 'The Microscope,' had given an illustration of the *Lepisma* scale, in which he showed that the appearance of dots produced at the lowest portion of the scale was owing to the corrugations on both sides of the scale crossing one another at an angle. He proved this very satisfactorily by putting Canada balsam on the upper or under side of the scale, and by so doing he obliterated the markings on that side where the balsam had run. He (Mr. J. Beck) therefore thought that it was shown that dots might be produced by lines running at an angle one to the other on either side of the membrane. Whether dots were always produced by the same cause was quite another matter. We might (reasoning from analogy) presume that they were formed in that way; but if we wanted to determine minute structures, and had been unable to do so by merely looking through the microscope, he apprehended the next best thing to do was to try experiment. Such experiment he had adopted so far as regarded the scale of *Lepidocyrtus*, and he was convinced that the appearances presented by that object were not the result of hemi-cylindrical tubes on the upper and under surface of the scale. That on one side of the scale there were gutters running from end to end there was no doubt. He had explained in his paper just published in the Journal, that this fact might be proved by merely breathing on the scale, and the moisture thus produced would run up and down on the exposed side. It was also clearly shown that no such gutters existed on the other surface of the scale; such being the case, there was the certainty that the appearances referred to were produced from some other cause. Did these note-of-exclamation markings really exist? If they were lines, how was it they did not appear as lines? If anyone looked through a piece of sheet glass at, say, the railings of a house, the distortions caused by the rays passing through the sheet glass gave an appearance of the breaking up of the railings all over. Whether the appearance produced in *Lepidocyrtus* could be explained in a similar way, he was not prepared to say. If we desired to ascertain minute structure, we must not totally depend on what we see through the microscope, but experiments must be conducted.

The great light that had been thrown on the structure of the Diatomaceæ was the result of experiment. The artificial deposition of silex had shown that the appearance visible on *P. angulatum*, &c., might be produced by hemispherical globules: that being a natural mode of deposition, it was fair to conclude that the structure was similar, unless future experiments should show that such a conclusion was wrong. As regard [illegible] ..., there was doubtless structure on

both sides, and this crossing of lines might produce the dotted appearance, and might account for the scale tearing across as readily as lengthways, which was not the case with *Lepidocyrtus*, which tore unevenly. If anyone was fortunate enough to get a large scale of *Lepidocyrtus* bent over, he would see all along the edge even ridges of the lines in profile, ridges which could not be seen even if they were due to the dottings of the notes-of-exclamation markings, which it was known did not run in lines across the scale, but in undulations, like watered silk.

A vote of thanks was then passed to Mr. McIntire for his paper.

Mr. W. S. Kent described briefly the contents of a paper "On a New Species of Sponge."

The thanks of the meeting were given to Mr. Kent.

Mr. Hogg read a communication from the President, "Note on Fluorescence *versus* Pseudo-dichroism."

The thanks of the meeting were given to the President.

Mr. Slack stated the contents of a paper "On an Optical Illusion Slide—Silica Films with Cracks." He had placed a slide in the Society's cabinet.

The next meeting of the Society will take place on 14th December.

Donations to the Library and Cabinet, November 9th, 1870:—

	From
Land and Water. Weekly	*Editor.*
Society of Arts Journal. Weekly	*Society.*
Nature. Weekly	*Editor.*
Journal of the Linnean Society, No. 49	*Society.*
Quarterly Journal of the Geological Society	*Society.*
Report of the Microscopic Objects in Cholera Evacuations. By F. R. Lewis, M.B.	*Author.*
Fossil Sponge Spicules in the Greensand of Haldon and Blackdown. By Edward Parfitt	*W. Vicary, Esq.*
The Royal Society in the Nineteenth Century. By Dr. A. B. Granville, F.R.A.S., &c.	*A Friend, through W. W. Reeves.*
A Photograph of a Double-centered Valve of Aulacodiscus Oreganus	*Dr. C. Johnstone, of Baltimore, U.S.*
Instructions for Collecting and Preserving Subjects of Natural History. By E. Donovon, F.L.S. 1805 ..	*Dr. Millar.*
Tractatus Quatuor Anotomici de Aure Humana. By J. F. Cassebohm. 1734	*Dr. Millar.*
Descriptio Anatomica Oculi Humani. By H. A. Wrisberg. 1780	*Dr. Millar.*
A Compleat System of Opticks. By Robert Smith, LL.D. 2 Vols. 1738	*Dr. Millar.*
Histoire Naturelle des Insectes Métamorphoses. Par Jean Goedaert. 3 Vols. 1700	*Dr. Millar.*
One Slide of Hydrodictyon utriculatum	*W. W. Reeves.*
One Slide of Silica Film with Cracks	*H. J. Slack, Esq.*

The following gentlemen were elected Fellows of the Society:—

Dannel Allen, Esq.
W. C. Puxty, Esq., F.G.S.E.
Joseph Kaines, Esq.
James William Groves, Esq.

WALTER W. REEVES,
Assist.-Secretary.

Quekett Microscopical Club.

At the meeting of the Quekett Club on the 23rd September, 1870, Dr. L. Beale, F.R.S., in the chair, four new members were elected, a number of presents to the library were announced, and thirty slides were presented to the cabinet. A paper was read "On so-called Spontaneous Generation," by Benjamin T. Lowne, M.R.C.S.Eng., &c.

The following is an abstract of this paper, which occupied an hour in reading. Mr. Lowne commenced by saying:—

When I announced a month ago that I would read you a paper on "Spontaneous Generation," I had no idea that one of the greatest living naturalists was going to give a most able *résumé* on the subject, or perhaps I should have hesitated in coming before you. Nevertheless I feel it is a matter for congratulation that I did so, as many unanswered questions have arisen since Professor Huxley delivered his able address at Liverpool.

Two hundred and two years ago Francesco Redi successfully combated the then prevalent doctrine of spontaneous generation by the most simple, nay, almost childlike experiments, such as putting meat under fine gauze, and so showing that maggots are not spontaneously generated. Since that day the tendency of experiments has certainly been in favour of Redi's aphorism, "Omne vivum e vivo."

The question, however, all turns upon that little word *omne*, all; whether all living things originate from germs, or whether some may originate spontaneously from not living matter.

Now, there can be no doubt but that there was a first cell and a first organism which had no progenitor. Professor Huxley said last week, that although he could not believe anything in the absence of evidence upon the subject, that "expectation is permissible where belief is not;" and that if it were given him "to look beyond the abyss of geologically recorded time to the still more remote period when the earth was passing through physical and chemical conditions, which it can no more see again than a man can recall his infancy," he "should expect to be a witness of the evolution of living protoplasm from not living matter."

To show you that I am not biassed in this matter, and that I am no partisan, I tell you I go farther in my expectation than Professor Huxley, and I think that if we could produce the conditions we might see amœbiform protoplasm originating even yet from inorganic matter. Perhaps, as Dr. Bastian suggests, colloid may be intermediate between inorganic and organic living material, but I tell you, gentlemen, this is all expectation, and should not be belief, as we have not at present a tittle of evidence in its favour. No doubt, with Mr. Charles Darwin's hypothesis, the origin of living organic matter from inorganic matter would supply a gap in the evolution of the animal kingdom, but we must not on that account found a scientific belief.

Now, gentlemen, I shall very carefully sift the supposed evidence in favour of spontaneous generation; I shall divide this evidence into that which is purely microscopic and that which is dependent on experiment.

First, with regard to the microscopic evidence, it consists in the assertion that some observers have seen organic living cells and fungus spores built up by the aggregation of minute granules. Now, there is very strong evidence on the other hand that this does not happen; the organisms described as fungus spores are in some cases not fungus spores at all, and in other cases they have been observed with a hilum or point at which they were attached to a parent. Surely, gentlemen, we cannot believe this point of attachment was the character of a spore formed *de novo*.

On the other hand, I should be sorry to deny, with my present knowledge, that it is possible organisms of a simpler kind, such as unicellular organisms, may be built up in this way. If such a mode of evolution does take place, I still believe it is from pre-existing germs; such gemmules, for instance, as Mr. Darwin believes in, in his beautiful provisional hypothesis of pangenesis.* I believe, if it can be proved that organisms can be produced by aggregation, it will be found that this only takes place when pre-existing cells have given up their contents in the fluid experimented on.

With regard to the experimental evidence, it has been arrived at from two classes of experiments.

The first aims at the production of known organic forms from solution of animal or vegetable matter, the second aims at the production of new and unknown forms under new conditions in saline solutions.

I shall consider these two sets of experiments separately.

In the first, or simplest set of experiments, the most contradictory evidence has been arrived at by different observers. The whole, to my mind, may, however, be summed up in the following.

If we receive the usually accepted belief that the boiling temperature destroys germs, we must accept spontaneous generation as a fact. If, on the other hand, we believe that germs are not killed in this manner, these experiments only show that if the greatest possible care is used, germs may not be admitted and a negative result may be arrived at, and yet that germs may find their way into the flasks of the most careful experimenter, and may afterwards germinate.

Now, gentlemen, I have instituted a series of the most careful experiments, which have shown conclusively to my mind that germs are not destroyed by the boiling temperature.

I took a neutral solution of acetate of ammonia and put into it a number of spores of the little mould known as *Penicillium glaucum*, and boiled them well. I then enclosed some of the boiled fluid and germs in capillary glass tubes, like those used for preserving vaccine lymph. I then carefully examined the tubes by scrutinizing them with the microscope for an hour each, and not a spore had germinated, not a mycelial filament existed in the tubes. I then put the tubes into a warm place by the stove, and in twenty-four hours numerous mycelial filaments of considerable length had protruded from many of the

* Mr. Lowne spoke at some length in favour of this hypothesis, and read that portion of Dr. Hooker's address to the British Association at Norwich, in 1868, upon the subject.

spores. Now, gentlemen, I should think the most hardy advocate of spontaneous generation would hardly assert that these spores had originated *de novo*, and germinated in a single night and day.

To make the experiment more complete, I enclosed in another tube some spores which had not been boiled, and I found about the same number had germinated in this tube as in those containing the boiled spores.

I have tried another set of experiments of a similar kind. I boiled a vegetable infusion containing a quantity of the bead-like growing mycelium of some fungus, probably a state of *Penicillium*, and mounted a few portions in a cell for the microscope. I then carefully examined and drew these portions, and watched them from hour to hour, and saw new cells formed and new buds put out. I have done this again and again with the same result.

I have further found that this process is arrested in sealed tubes after a few hours; I cannot tell why, but I strongly suspect from the absence of dissolved air in the fluids: Mr. Cooke has suggested it may possibly be from the absence of dissolved nitrogen. I strongly suspect it is from this fact that we are able to preserve meats, &c., *in vacuo*.

Of this at least there can be no doubt, both the growing mycelium and the spores of the common blue mould, *Penicillium glaucum*, will grow after boiling, and it is nevertheless possible to preserve meat, &c., on a large scale, by enclosing it *in vacuo* after boiling it.

I may here remark that Dr. Bastian's eighth experiment,* in which he found that an infusion of turnip decomposed more rapidly when enclosed *in vacuo* than a similar solution enclosed in a flask containing air, is simply incomprehensible, and is a contradiction to the well-known process of preserving meats, vegetables, fish, &c.

I think, gentlemen, very few will believe we are justified, *without evidence*, in believing a temperature somewhat higher will kill these spores if boiling does not. I therefore look upon it that if a few observers will repeat my very simple experiments, no evidence is afforded by such experiments as those I have included under this first division in favour of generation *de novo*.

The second series of experiments which aim at the production of new and unknown organisms, afford a wider field for speculation. I must confess, however, that in every case which I have seen, these so-called new organisms have appeared to me undoubtedly foreign bodies, which have accidentally gained access to the solutions.

The most recent experiments of this kind were carried out by Dr. Bastian, and their results have been published in 'Nature.' In these experiments a solution of sodic-phosphate and ammoniac carbonate was enclosed *in vacuo* whilst boiling, and certain spiral fibres and portions of a fungus, like penicillium in fruit, were found after a time in the solutions.

With a view to discover whether the spore-bearing portions of *Penicillium* would stand boiling, I tried the unripe spore-bearing filaments, and found that they were not altered in their appearance by such treatment. The ripe spores are, however, immediately scattered

* 'Nature,' Pt. xxxvi., p. 194.

by contact with fluid. Now I can readily understand why these fungi were not discovered until after a long lapse of time: I find solutions of sodic phosphate throw down a flocculent precipitate after a time, and in those specimens which Dr. Bastian was courteous enough to show me, I observed that the object was surrounded by just such a precipitate, which he called correctly enough granular matter. I suspect the collection of such a flocculus around the fungus drew his attention to the spot where the minute mass of fungus was.

Another reason for not believing that the fruit-bearing stems of *Penicillium*, which Dr. Bastian figures, were formed in the solutions, is that these fungi never fructify in fluid. My friend Mr. M. C. Cooke tells me that he never heard of any fungi, except such as are parasites on insects, fructifying in fluid, or so long as a plentiful supply of fluid is present. As he very forcibly put it, take the vinegar plant as an example; so long as there is plenty of fluid, it never produces fruit; but take it out of the fluid, and its surface will soon be covered with blue mould. With regard to the so-called spiral fibre organisms of Dr. Bastian, they have puzzled me very much. I never, however, believed but that they were some very common accidental material which had found its way into his solutions.

I observed that he only found these "organisms" in solutions containing sodic phosphate. I have tested and had tested for me three samples of crystals of this salt, and in all free soda was present. I have since tried the action of very dilute solutions of caustic alkali on various kinds of organic fibre, and have found wool fibres, minute particles of feathers, and some kinds of spiders' thread twist into spirals under its influence. Now, the spirals produced from spider's silk correspond most closely with Dr. Bastian's spiral fibre. In my own mind I have no doubt the specimen he kindly showed me was spider's silk.*

At any rate, gentlemen, I do not think in the face of this we ought to conclude that we have discovered spontaneous evolution from the appearance of spirals in an alkaline solution.

I apprehend then, sir, from what I have said, if my experiments are confirmed, which can easily be done, that at present, let our "philosophic faith be what it may," we have no evidence whatever of spontaneous evolution.

A discussion followed entirely in favour of Mr. Lowne's views on the insufficiency or absence of any evidence in favour of spontaneous generation, in which the President, Mr. M. C. Cooke, and others took part. The President, having left the chair for the purpose, also spoke at some length upon the subject, demonstrating with great clearness several important errors into which Dr. Bastian must have fallen, and disposing in a masterly manner of a number of arguments advanced and relied upon by the advocates of spontaneous generation. The results of some experiments by Dr. Child, which were made at the

* My friend Mr. Charles Stewart tells me one species of common spider spins twisted silk. I have not seen this; but I found the silk of *Epiera diadema*, the common garden spider, sometimes twists into beautiful spirals under the influence of dilute alkaline solution.

laboratory of the University of Oxford during the previous week in repetition of those of Dr. Bastian, were communicated to the meeting, and the question was declared to resolve itself merely into one of the exclusion of germs. A number of other points of great interest as bearing upon the general subject were touched upon by the speaker, who was listened to throughout with the greatest attention, and resumed his seat amidst great applause. A cordial vote of thanks to Mr. Lowne for his very able and important paper was carried unanimously, and the proceedings terminated, as usual, by a conversazione, at which microscopical objects of interest were exhibited by Messrs. Conder, Hainworth, Meacher, Quick, Slade, and T. C. White.

Brighton and Sussex Natural History Society.*

August 25th. Microscopical Section. Mr. T. H. Hennah, President, in the chair.—An evening on mounting microscopic objects, when practical instruction in some of the different methods of mounting objects for the microscope was given by the following gentlemen:—

Mr. Hennah showed how to mount diatoms as day objects, in the course of which operation he gave a caution against using a pipette in taking them out of the store bottle, giving preference to a glass rod.

He next illustrated the mode of mounting in glycerine jelly, one of the most valuable agents in preserving animal tissue or vegetable preparations; and pointed out that objects could be temporarily preserved in it, and laid by without fear of injury, until time could be found for finishing the preparation.

Dr. Hallifax gave instruction in this method of making shallow or deep cells for objects mounted in fluid, and detailed the experiments he had made with different substances to form a cell of any thickness, which should be permanent, and, at the same time, not be acted upon by the medium in which mounted. He had found a mixture of marine glue and shellac gave the best results.

Several ingenious contrivances for removing minute objects from fluid, floating them on the slide in the exact position required, as well as a modification of the turn-table for enabling the brush to be held steadily in a perpendicular manner, were also shown by Dr. Hallifax.

Mr. Wonfor showed how to fix cells with marine glue, and illustrated the process of mounting dry objects, from the fixing the cell to the finishing the slide ready for the cabinet. For dry objects he had found nothing so permanent as glass cells, or, what was cheaper, brass rings, either of which could be easily attached by marine glue, the most trustworthy cement for fixing. He had tried a great variety of cells, and found objections to all but glass and metal.

Mr. Wonfor also exhibited one of Dr. Mathews' improved turntables for making shallow cells, and pointed out its superiority over the old form.

The comparative value of different cements was discussed, and several practical hints in minutiæ of mounting and preparation were given by the same gentleman during the evening.

tary.

September 8th. Annual meeting. Mr. T. H. Hennah, President, in the chair.—The following officers for the ensuing year were elected:— President, Mr. F. Merrifield; Committee, Messrs. R. Glaisyer, J. Dennant, G. D. Sawyer, Haselwood, Rev. J. H. Cross, and Dr. Badcock; Treasurer, Mr. T. B. Horne; Hon. Secretaries, Messrs. T. W. Wonfor and J. C. Onions; Hon. Librarian, Mr. Gwatkin. The name of Mr. T. H. Hennah, the out-going President, was added to the list of Vice-Presidents.

By the Committee's report it appears that the Society is in a flourishing condition, there being a balance of 1*l*. 19*s*. in the hands of the Treasurer, after expending 16*l*. 19*s*. in the purchase of new books and periodicals, and in the publication of the 'Moss Flora of Sussex.' By purchase and donations the library has been increased to 643 volumes. During the year a microscopical section was formed experimentally with such success that its continuance was recommended, the effect of which is to make the meetings of the Society bi-monthly, the second in each month being a microscopical one. Two active members of this Society, Messrs. W. Verrall and T. Eden (the latter a Vice-President), had been removed by death. The thanks of the Society were due to those who had read papers, exhibited specimens, microscopes, and contributed to the library and cabinet.

From the abstract of the 'Proceedings' it appears papers on Mosses and on the Gemmæ of Mosses had been read by Mr. C. P. Smith; on Volcanic Theories and the Presence of Copper in Coal-Gas, by Mr. J. E. Mayall; on Diptera and their Wings, by Mr. Peake; on Soundings, made by Sir E. Parry in the Arctic Seas in 1818; and on Systematic *Recent* Examination with moderate Powers, by Mr. T. H. Hennah; on British Gall-Flies and their Parasites, by W. H. Kidd; on Sussex Centenarians, by Mr. J. Robertson; on the Vertebrate and Invertebrate Eye, compared by Dr. Hallifax; and on Seeds, the Attractive Power of Female Moths, Infusoria, The Annual Excursion, and the Eggs of Articulata, by Mr. T. W. Wonfor. In addition there had been six microscopical meetings and two meetings for the exhibition of specimens. There had been six Field Excursions and a Dredging Expedition, in addition to the Annual Excursion. Votes of thanks were passed to the Officers of the Society and to the Medico-Chirurgical Society for permission to use the room at the Dispensary.

The meeting then became general for the exhibition of specimens, when Mr. F. Merrifield, the newly-elected President, took the chair.

Mr. Robertson wished some record could be preserved of some of the ingenious methods of microscopical manipulation, shown at the last meeting by Messrs. Hennah, Wonfor, and Dr. Hallifax. Messrs. Hennah and Wonfor explained that as far as practicable such had been done; but many things shown were to be found well explained in Carpenter and Quekett.

Mr. Pocock exhibited a specimen of *D. Galii* (the bed straw hawk moth), caught by his father at Kemp Town.

Mr. Wonfor remarked that, owing to the very dry summer, this rare moth and *D. livornica* (striped hawk moth) had been plentiful, about twenty specimens of the two having been captured near Brighton, as well as three larvæ taken, from which moths had been bred.

Mr. Wonfor exhibited dwarf specimens, with one exception caught, of several butterflies—these showed that there were dwarfs in Nature as well as those produced by rearing artificially; and many blue varieties of the females of three species of the blue butterfly.

Mr. Merrifield stated that, as a rule, bred specimens were smaller than caught ones.

Messrs. Howell and Wonfor exhibited specimens of Websterite, Allophane, Selenite, and various other peculiar minerals, obtained in excavations for the main drainage works in Vernon Terrace and Clifton Hill.

Mr. Ardley exhibited jet, ammonites, belemnites, and impressions of leaves, obtained by him at Whitby.

Dr. Badcock exhibited seaweed (*Sargassum vulgare*), obtained by him in the Gulf Stream in 1864.

Mr. C. P. Smith exhibited specimens of *Sargassum bacciferum*, washed ashore on the west coast of Ireland.

Mr. Wonfor exhibited amber, containing a four-winged fly and different ores of copper, from the Burra-Burra Mine, Australia.

September 22nd. Microscopical meeting. Mr. T. H. Hennah, Vice-President, in the chair.—"On Sections and Section Cutting."

Dr. Hallifax gave practical instruction in his method of making sections of animal and vegetable tissues. Starting with a Beck's section-cutting instrument, intended especially for making wood sections, he found the accurate workmanship and delicate screw enabled him to cut sections $\frac{1}{1000}$th of an inch thick.

For soft substances, with the gun-metal blocks supplied with the instrument and a strip of paper, he contrived to form a cell, in which melted wax was poured; in this the object to be operated on, previously coated with gum and dried, was arranged. By using a razor instead of the chisel, he was now able to make very thin sections, the graduated screw enabling him to determine their thickness. On floating the sections in water the coating of gum dissolved, and cleared them of the surrounding wax.

By such a method he had made sections of brain, spinal chord, animal and vegetable tissues, seeds, leaves, insects, &c.; specimens of which, and of the retina of the eye of a sheep, were handed round as illustrations.

Mr. Hennah then showed how, with the same instrument, sections of wood or the stems of plants could be made by fixing them in the gun-metal blocks with paraffine wax before cutting.

Mr. Wonfor illustrated the making of sections of harder substances, such as the stones of the peach or the coquilla nut. A thin slice was first cut with a fine saw, ground on a coarse stone, and finished on a water-of-Ayr stone with water.

He then described and performed a process recently devised by Dr. Ormerod, of Brighton, for making and mounting sections of bone. A thin slice was cut with a fine saw; this was roughly ground on a coarse stone, and finished with a piece of flat pumice-stone on a plate of ground glass, with water. After well washing the section, only the

superfluous moisture was removed with blotting-paper. Canada balsam was then boiled, both on a glass slide and a covering glass. When the balsam was nearly cold, the bone was arranged on the slide, and the covering glass laid on; the whole was now warmed, and the glass cover pressed down. By this means the structure of the bone was admirably shown. A hot knife having been run round the edge of the covering glass, the superfluous balsam could now be easily cleaned off in cold water.

From the cutting the slice of bone until the slide was labelled and ready for the cabinet, less than half an hour had elapsed—a very great advantage, to those who wished to study osseous structure, over the ordinary methods of preparing sections.

Thin sections of wood, cut with a razor instead of a chisel, shells of nuts, and bone, were handed round as illustrations.

It was announced that the next microscopical meeting would be on October 27th; the subject—"Illumination."

October 13th. Ordinary meeting.—Mr. T. H. Hennah, Vice-President, in the chair.

The Secretaries of the Quekett Microscopical Club, the Lewes Natural History Society, and the Maidstone and Mid-Kent Natural History Society, were elected Honorary Members of the Society.

Dr. Badcock read a paper "On the Gulf Stream," in which the peculiar phenomena, nature of its current, possible cause of its origin, with an account of some of the forms of life met with in the Gulf Stream, were described and discussed.

Reading Microscopical Society.*

October 18th, 1870.—After the usual business incidental to the opening of a fresh session, Captain Lang, the President, read a very practical and useful paper "On Selecting and Mounting Diatoms," in which he urged the importance and desirability of making separate mounts of the species of diatoms found in any particular locality. He described Captain Haigh's method of making cells, &c., and gave full particulars of his own and Mr. Tatem's methods of picking out and mounting selected specimens, as well as of the various media and cements employed. Specimens of slides and mounts in their various stages were shown as illustrations.

Captain Lang also brought before the notice of the meeting Mr. Baker's form of dissecting compound microscope with erecting eyepiece prism, and also Gundlach's $\frac{1}{8}$th, $\frac{1}{12}$th, and $\frac{1}{16}$th object-glasses.

Mr. Tatem exhibited balsam mounts of minute *Acari* (obtained from a dead flea of the ferret) having *four* legs only. These are shown to be immature forms; two pairs of well-developed posterior legs, ready for evolution at an approaching moult, being clearly discernible, determining the fact that some *Acari* are excluded from the egg with but *four* legs, subsequently acquiring the normal *eight* in the progress of growth.

* Report supplied by Mr. B. J. Austin.

At the July meeting of the Entomological Society, Professor Westwood made some observations on minute *Acari* found in the unopened buds of black-currant trees, and possessing but *four* legs, and on another species, which forms small pustules on the leaves of pear-trees; these, with a third form, described some years since in France, he regarded as constituting a distinct tribe of the family *Acaridæ*, and must at all events be placed in a separate genus, for which he proposed the name of *Acarellus*.

Mr. Tatem also exhibited the flea of a ferret and gizzard of an *elater*.

Mr. Austin showed *Hydrodictyon utriculatum* (in three stages), *Zygnema* after conjugation (with spores in two stages), *Peziza villosa* (on *Ulex*), and *Uncinula vicornis*, *Peronospora macrospora*, *Lecythea mixta* (leaf fungi).

November 1st.—The usual business of the annual meeting having been transacted, a paper by Mr. Tatem was read. In "Notes on New Infusoria," he described *Difflugia ligata*, aberrant forms of *Stylonichia* and *Epistylis* ———, of which full particulars were given. Camera drawings illustrated the paper.*

Captain Lang read "A Note on *Actinophrys Sol*," recording observations made on the 2nd of October upon two of these animals, one about twice the size of the other. They were distant from each other some two or three diameters of the larger one, but were connected by *one* of the pseudopodia. Gradually the smaller *Actinophrys* was drawn close to the larger one, and as this took place the smooth tapering pseudopodia of both became irregularly moniliform. The animals remained close to each other for about five minutes, and then slowly resumed their former position; the moniliform appearance of the pseudopodia passing away. Two or three minutes afterwards the larger one emitted from the side nearest its companion a swarm of very minute quivering atoms. The power used was one of 280 diameters, obtained by Beck's $\frac{4}{10}$th objective (on the double nose-piece), and his *second* eye-piece. In the short time required to get a $\frac{1}{8}$th objective, and put it on to examine these small bodies, they had disappeared. The two creatures once more approached, but nothing particular was observed. It was then seen that the larger *Actinophrys* was altering its form from a circular to a dumb-bell shape. The constriction, on which there were no pseudopodia, rapidly increased, became at last a mere thread, and eventually complete fission took place. Two separate and perfect *Actinophrya* were the result, each being exactly the same size as the smaller one first observed, and which was still in the field of the microscope.

The self-division had been often witnessed, but the prior proceedings here described the writer had never seen or heard of before, and hence the record, without any attempt to give an opinion or explanation.

Mr. Hoyle exhibited a seven-lobed *Floscularia* (? *Floscularia proboscidea*).

* This paper will be found in the present Number.

Microscopical Society of Liverpool.

The fifth ordinary meeting was held at the Royal Institution on May 6th, Rev. W. Banister, B.A., in the chair.—A paper was read by Dr. T. R. Glynn, "On Ciliary Motion."

The author of the paper commenced by describing the structure of cilia, and alluded to the different opinions as to whether the cilium was a prolongation of the cell wall or continuous with its contents. He then described the motion of cilia; noticed the apparent co-ordination in the movement in cilia on the same and on different cells, &c., the greatness of the force exerted by cilia, and attempted to show that none of the theories advanced to explain the mechanism of ciliary motion were satisfactory. He alluded to the different forms which illustrated the conversion of vital into physical force, as displayed in ciliary motion. He then endeavoured to show how, when submitted to the action of various agents, the motion of cilia differed from that of the yolk in the ova of fish, from the movement seen in the cells of plants, and from molecular motion. Having described the arrangement of cilia in several of the infusoria and rotifera and certain vegetable forms, the author briefly considered their distribution and functions in the animal kingdom generally. The meeting concluded with the usual conversazione.

At the sixth ordinary meeting of the Microscopical Society of Liverpool, held June 10th, a paper was read by the Rev. W. Banister, "On Microscopic Fungi."

After an introductory apologetic plea for fungi, on the ground of their beautiful and interesting structure when seen under the microscope, he gave a general sketch of the mode of growth and propagation, and then proceeded to describe some members of the great family of Rust, Brand, Blight, Smut, Mildew, and Mould, which were illustrated by mounted specimens. He desired particularly to call the attention of microscopists to the importance of the study of microscopic fungi, with a view to discover a means of preventing the injury they cause to corn and other crops, and, indeed, to every vegetable which they attack. He stated that the ordinary spores were too large in most cases to enter the stomata of the leaves, or the spongioles at the extremity of the outlets, and that although the secondary spores, as they might be called, *i.e.* the minute vegetative cells produced on threads thrown out by the true spores, might enter, yet these are not known to exist in all species, and there are innumerable difficulties in the way of accounting for their entrance by this process. He quoted a calculation from Mr. C. Cooke's work, estimating the possible fecundity of *Œcidium tragopogonis*, *e.g.* 2000 cluster-cups have been seen on one moderate-sized leaf of Goatsbeard—every cup contains 250,000 spores, each of which may produce a number of smaller vegetative spores—so that, not taking these into account, one leaf may carry 500 million reproductive bodies. The same might be said of Bunt, Corn, Mildew, &c. The question then is, How do these spores enter the tissues of the plant or inoculate the young plants of next year? For even the seed-leaves have been found infected.

(*To be continued.*)

Oldham Microscopical Society.

On Tuesday evening, the 18th October, at No. 2, High Street, a paper was read before the members of the above Society, by Mr. James Nield, "On British Ferns, microscopically considered." The paper was illustrated by diagrams showing the parts much enlarged, and by a complete set of dried specimens, representing each genus, also by numerous specimens of the microscopic parts of ferns carefully mounted for observation, and which, after the paper was read, were submitted to very critical examination under several first-class binocular instruments, and called forth many expressions of wonder and delight. At the close a well-earned vote of thanks was accorded to Mr. Nield for his paper, which is one of a series to be read during the winter months.

Tunbridge Wells Microscopical Society.*

The meeting of this Society took place on November 8, at the house of John Field, Esq. The subject was "Hairs, Animal and Vegetable." Some very curious and interesting objects were exhibited, illustrating the subject for discussion.

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* From the Rev. B. Whitelock, Hon. Secretary.

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